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The anomaly called psi: Recent research and criticism

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Abstract: Over the past hundred years, a number of scientific investigators claim to have adduced experimental evidence for "psi" phenomena – that is, the apparent ability to receive information shielded from the senses (ESP) and to influence systems outside the sphere of motor activity (PK). A report of one series of highly significant psi experiments and the objections of critics are discussed in some depth. It is concluded that the possibility of sensory cues, machine bias, cheating by subjects, and experimenter error or incompetence cannot reasonably account for the significant results. In addition, less detailed reviews of the experimental results in several broad areas of psi research indicate that psi results are statistically replicable and that significant patterns exist across a large body of experimental data. For example, a wide range of research seems to converge on the idea that, because ESP "information" seems to behave like a weak signal that has to compete for the information-processing resources of the organism, a reduction of ongoing sensorimotor activity may facilitate ESP detection. Such a meaningful convergence of results suggests that psi phenomena may represent a unitary, coherent process whose nature and compatibility with current physical theory have yet to be determined. The theoretical implications and potential practical applications of psi could be significant, irrespective of the small magnitude of psi effects in laboratory settings.

Keywords: clairvoyance; extrasensory perception (ESP); methodology; parapsychology; psi; psychokinesis (PK); replication; scientific method; telepathy

1. Introduction

There is a large and growing body of experimental literature devoted to the study of certain anomalous interactions that seem to involve psychologically meaningful exchanges of information between living organisms and their environment. We call these interactions *anomalous* because they appear to exceed somehow the capacities of the sensory and motor systems as these are presently understood. These interactions are collectively designated by the term *psi*. *Parapsychology* is that branch of science that makes a systematic study of psi anomalies. In other words, it is the business of parapsychology to find explanations of psi anomalies through scientific inquiry.

Psi is traditionally divided into various subcategories, each of which has been the subject of experimental research. For example, parapsychologists have been testing whether subjects can acquire information that is shielded from their senses (*extrasensory perception*, or ESP) and whether subjects can directly influence external systems that are outside the sphere of their motor activity (*psychokinesis*, or PK). Experimenters have also sought to differentiate forms of ESP, such as "telepathy" (ESP for another's thoughts) and "clairvoyance" (ESP for external objects and events). ESP is sometimes reported to be time-displaced, in that the information may relate to a past event ("retrocognition") or a future event ("precognition"). In practice, it has often proved difficult to isolate these forms of psi experimentally, and nowadays they tend to be defined operationally rather than theoretically (e.g., it is clairvoyance when you do not have someone "sending" the target).

Somewhat contrary to common usage, we are not using the term *psi* to imply that the anomalous interactions are necessarily "paranormal," but rather that no *adequate* conventional explanation of the interactions has yet been offered. Phrases stating or implying the "existence" of psi will be used somewhat informally to indicate that certain interactions have achieved this status.

The term *paranormal* has been a source of some confusion both within and outside parapsychology, and thus we feel that a few comments on the term are in order. Paranormal was first discussed in relation to psi by the philosopher C. D. Broad (1953; 1962; see also Braude 1979b), who defined psychical research (the earlier term for parapsychology) as "the scientific investigation of ostensibly paranormal phenomena" (Broad 1962, p. 3). Broad was careful to use the term "ostensibly paranormal," by which he meant phenomena that seem prima facie to conflict with one or more of what he referred to as the "basic limiting principles" of nature. These are not the same as the laws of nature, but rather a more fundamental set of assumptions that "we unhesitatingly take for granted as the framework within which all our practical activities and our scientific theories are confined" (Broad 1953, p. 7). For example, the assumption that "it is impossible for a person to perceive a physical event or a material thing except by means of sensations which that event or thing produces in his mind" (Broad 1953, p. 10) is a basic limiting principle that governs our way of acquiring knowledge. A case of ESP, therefore, would be *ostensibly* paranormal; it would be *genuinely* paranormal only when and if it could be shown to really conflict with one or more of the basic limiting principles. It is the task

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of parapsychology, according to Broad, "to investigate ostensibly paranormal phenomena, with a view to discovering whether they are or are not genuinely paranormal phenomena" (Broad 1962, p. 5).

Although Broad's reasoning is sound, the term *paranormal* has led to some difficulties in practice. For example, as noted above, it is commonplace to find the terms *psi* and *paranormal phenomena* being used interchangeably, implying that parapsychology has no subject matter unless the paranormality of the phenomena is accepted in advance! A second and subtler difficulty is more directly related to the term itself. By stressing the conflict between potential "paranormal" explanations of psi and "normal" science, and at the same time failing to acknowledge that what constitutes normal science is historically relative (i.e., it can change from one historical period to the next), the term *paranormal* leaves the connotation that explanations that violate the basic limiting principles are unscientific in some fundamental sense. This, of course, is not true. If a "paranormal" theory of psi were someday to be confirmed, the practical consequence would be a redefinition of "normal" science to accommodate the new theory. In other words, the "paranormal" would become "normal," and the distinction would break down. A similar objection to the term has recently been raised by Paul Kurtz (1981), a well-known critic of parapsychology.

It is our view that potential explanations of psi that violate the basic limiting principles of nature are scientifically legitimate and, along with conventional explanations, should be entertained from the outset in our efforts to explain psi anomalies. Such explanations, unorthodox as they may be, are nonetheless worthy of consideration for the simple reason that psi anomalies seem to violate the basic limiting principles prima facie. Things are not always what they seem, but the possibility that they are should certainly be considered. Thus, the distinction to which *paranormal* refers is a valid one, even though the term itself is problematic. Recently, Palmer (1986b) has proposed a neutral term, *omega*, to identify potential explanations of psi that go beyond the basic limiting principles. Thus, "paranormal" explanations would be labeled "omegic." Despite our reluctance to introduce neologisms, we think in this case an exception may be justified.

2. Background

Like conventional psychology, experimental parapsychology grew out of a need to account for people's experiences in the "real world." The first major survey of such experiences was conducted under the auspices of the British Society for Psychical Research in the last century (Gurney et al. 1886/1970). More recently, a survey conducted by the National Opinion Research Center of the University of Chicago revealed that a majority of Americans thought they had experienced one or more psychic events in their lives (Greeley & McCready 1975). Similar results have been obtained in other surveys in the United States (e.g., Palmer 1979), Europe (e.g., Green 1960; Sannwald 1963; Haraldsson et al. 1977), and Asia (e.g., Prasad & Stevenson 1968). Palmer's survey further revealed that for many of those who reported psychic experiences, these significantly influenced their feelings, attitudes, and decisions in other areas of their lives. Whatever the explanation of psychic experiences, they happen, they are common, and they are often important to people. For these reasons alone, they deserve serious attention from scientists involved in the study of human behavior and cognition.

Although some parapsychological research has directly examined the evidential value and characterization of these spontaneous psychic experiences (e.g., Hart 1954; Rhine, L. E. 1962; Schouten 1982), the bulk of the research has been experimental, and we will limit ourselves to the latter in this target article. The first major experimental investigation of psi was conducted at Stanford University by John Coover (1917). Sustained research, however, did not begin until 1927, when J. B. Rhine arrived at Duke University to work with William McDougall. With the publication of J. B. Rhine's (1934/1973) monograph *Extrasensory Perception*, a scientific claim for the existence of ESP was made. It gave the field "a shared language, methods, and problems" (McVaugh & Mauskopf 1976), and it provided "radical innovation and a high potential for elaboration" (Allison 1973, p. 39).

Rhine's procedure was to have subjects guess the randomized order of the cards in a deck containing five examples of each of five geometric symbols: a star, circle, cross, square, and wavy lines. By chance, the subject should get 5 correct out of the 25. Standard statistical techniques were used to determine the likelihood that any given number of hits was statistically significant. If the average number of correct guesses per run of 25 exceeded 5 to a significant degree, and acquisition of information by artifactual means such as sensory cueing and logical inference was ruled out, ESP was considered to have been demonstrated.

Using this methodology, Rhine (1934/1973) reported highly significant results, especially with five selected subjects who were tested repeatedly over a number of years. Prior to August 1, 1933, all subjects in the program had completed a grand total of 85,724 trials, with an average score of 7.1 hits per run.

The reaction of the scientific community to Rhine's claim was understandably cautious and critical. Subsequent to the publication of the monograph, there were 35 criticisms contained in 56 published reports. Some of these criticisms were specific and others were merely speculative. The specific criticisms had to do with Rhine's methods of data collection and statistical analysis. These criticisms and Rhine's responses are fully documented in the book *Extrasensory Perception After Sixty Years* (Rhine et al. 1930).

The first line of criticism dealt with the experimental conditions. One essential requirement for an acceptable ESP experiment was that data should be collected under conditions that provide no reasonable opportunity for sensory leakage of information or inferential knowledge of the targets. Skinner (1937), Wolfle (1938), and J. L. Kennedy (1938), among others, pointed out that under certain lighting conditions the commercially produced ESP cards could be read through their reverse sides. Rhine responded that the original experiments were conducted with hand-printed ESP cards that were free from such defects and that in his more formal experiments the use of screens and distance prevented the subjects from obtaining any visual cues from the cards. Kennedy (1938), Kellogg (1936), and Leuba (1938) argued that an increase in the experimental rigor of ESP research had resulted in a corresponding decline in ESP results, suggesting that extrachance ESP scores were due to loose experimental conditions. To this Rhine responded that his most rigorously controlled experiment, the Pearce-Pratt series, did give highly significant results (Rhine et al. 1940). Although this experiment was later challenged by critic C. E. M. Hansel (1966) – with questionable success (Hansel 1980; Rhine & Pratt 1961; Stevenson 1967) – as being susceptible to fraud on the part of the subject, it was still more rigorously controlled than the other experiments in the original data base and thus supported Rhine's point.

The second line of criticism related to data analysis. Willoughby (1935), Kellogg (1936), Heinlein and Heinlein (1938), Herr (1938), and Lemmon (1939) criticized various features of the statistical analysis used by Rhine and his colleagues. In particular, the criticism focused on Rhine's assumption that the binomial theorem is applicable to "closed decks," decks in which the number of times each type of card appears is not free to vary. This aspect of the methodological debate essentially ceased in 1937, when Burton Camp, President of the Institute of Mathematical Statistics, stated that Rhine's "statistical analysis is essentially valid. If the Rhine investigation is to be fairly attacked it must be on other than mathematical grounds" (Camp 1937). For further details, see Burdick and Kelly (1977).

It would be wrong to conclude from this, however, that Rhine's experiments were perfect and that they had conclusively eliminated every alternative explanation. In retrospect, one could suggest improvements in the experimental conditions of his experiments. But for his time, Rhine's best experiments were ahead of others in the behavioral sciences. The experimental precautions he took, including two-experimenter controls and double-blind procedures, were rare in other disciplines at that time. Nonetheless, much of the early criticism of Rhine's experiments was helpful in progressively raising the standards of ESP research and reducing the possibility of experimental errors and artifacts.

Since the publication of Rhine's monograph over fifty years ago, there have been hundreds of experimental reports of evidence for psi. Yet skepticism has not decreased. Psi results are generally ignored in mainstream science, and when called to the attention of scientists they are apt to arouse suspicion. When specific criticisms are voiced, they generally include the following: (1) There is no "conclusive" experiment in parapsychology's long history; (2) there is no repeatable psi experiment; (3) the so-called significant psi results are disparate, incoherent, and isolated one-shot observations that do not merit scientific attention; (4) the results themselves are nonsensical in that they do not suggest any lawful relationships or progressive research programs; and (5) even if psi is real, it is too weak to be of any practical importance. If such perceptions were strongly supported by all the available data, it would be right to ignore parapsychology's claims. But the fact (as we hope to show in the following pages) is that (1) there are good experiments that seem to provide evidence for the existence of psi by reasonable standards of judging such evidence; (2) there are results that enjoy a respectable rate of replication; (3) the experimental observations in parapsychology are not unrelated, and significant patterns involving large bodies of experimental data are apparent; (4) a wide range of process-oriented research has focused on a single cognitive process that may be seen to give coherence and even a degree of consistency to a diverse array of experimental results; and (5) the small magnitude of most current psi effects is irrelevant to both their theoretical importance and their potential applicability.

3. The question of the "conclusive" experiment

Referring to parapsychology, Phillip H. Abelson (1978), Editor of *Science*, is quoted in *U.S. News and World Report* as saying that "extraordinary claims require extraordinary evidence." This statement implies that the strength of evidence required to establish a new phenomenon is directly proportional to how incongruent the phenomenon is with our prior notions. Our prior notions, however, are not always self-evident truisms. They are derived from, among other things, prevailing religious and cultural beliefs, personal experiences and observations, and our general world view. They are translated into subjective probability estimates and determine the evidential demands we make for a given claim. If the subjective probability of a disputed claim is zero, then no amount of empirical evidence will be sufficient to establish that claim. In serious scientific discourse, however, few would be expected to take a zero-probability stance because such a stance could be seen to be sheer dogmatism and the very antithesis of the basic assumption of science's open-endedness.

Nevertheless, the demand for extraordinary evidence of psi often seems to be derived from an implicit notion of its a priori impossibility. For example, some critics of psi research have demanded a "foolproof" experiment that would control for all conceivable kinds of error, including fraud by the experimenter(s). They have argued that if a claim is made for the existence of a phenomenon that conflicts with "established laws," it is much more parsimonious to assume error or even fraud on the part of the claimant than it is to assume the reality of that phenomenon (Price 1955; Hansel 1966). This argument is often identified with David Hume's (1825) maxim that "no testimony is sufficient to establish a miracle, unless the testimony be of such a kind, that its falsehood would be more miraculous than the fact which it endeavours to establish" (p. 115). Hume's maxim is a metaphysical statement, and it is inappropriate to use it when one speaks of empirical evidence. Moreover, his definition of a miracle as a universally nonexistent event is self-contradictory inasmuch as any claimed evidence in support of a miracle is also evidence against the universality of its nonexistence (Rao 1981a). As Saint Augustine remarked, "Miracles occur in contradiction not to nature, but to what is known to us of nature." It should also be kept in mind that Hume might not have regarded psi phenomena as miraculous or as anything more than extraordinary events.

The call for a totally "foolproof" study assumes that at a given time one can identify all possible sources of error and how to control against them. Such a methodological stance is comparable to the epistemological position that one can determine for all time to come what is and is not possible. Again, the demand for experimental controls against experimenter fraud is unique to discussions of evidence for what are perceived to be extraordinary claims. Pushed to its extreme, the hypothesis of experimenter fraud becomes nonfalsifiable, in that it is impossible to be certain that fraud is completely eliminated in any given experiment.

The concept of a "conclusive" experiment, totally free of any possible error or fraud and immune to all skeptical doubt, is a practical impossibility for empirical phenomena. In reality, evidence in science is a matter of degree; the fact that one can concoct alternative explanations of a finding does not automatically render that finding evidentially worthless. Evidentiality must be assessed on a continuum and in relation to the plausibility of and the empirical support for the competing hypotheses. These considerations demand that a "conclusive" experiment be defined more modestly as one in which it is highly *improbable* that the result is artifactual. In *this* sense, we think a case can be made for "conclusive" experiments in parapsychology.

3.1. Schmidt's REG experiments

A defense of the existence of probabilistically conclusive parapsychological studies requires a detailed review and discussion of any experiments that might qualify. Because such a treatment must be rather lengthy, we will limit ourselves to a single group of experiments as an example. Although they are somewhat dated, we have chosen Helmut Schmidt's (1969a; 1969b) reports on random event generator (REG) experiments because (a) they represent one of the major experimental paradigms in contemporary parapsychology; (b) they are regarded by most parapsychologists as providing good evidence for psi; and (c) they have been subjected to detailed scrutiny by critics. In no sense do we imply that these are the only good experiments the field has to offer. Nor do we believe, for the reasons stated above, that there can be any crucial experiment or experimental program on which the case for psi does or could rest exclusively.

At the time of conducting these experiments, Helmut Schmidt was a physicist at Boeing Scientific Research Laboratories. The studies were designed to test the possibility of ESP and were carried out with the help of a specially built machine that seemed to rule out all artifacts arising from recording errors, sensory cues, and subject cheating. The safety features of the Schmidt machine are actually superior to those of the VERITAC machine used earlier by Smith and his colleagues to test for ESP (Smith et al. 1963). Hansel (1966) had praised VERITAC as "admirably designed" and had suggested that it could be "standardized for testing subjects for extrasensory perception" (p. 172).

The Schmidt machine randomly selected targets with equal probability and recorded both the target selections and the subject's responses. The subject's task was to guess which of four lamps would light and to press the corresponding button if he was aiming for high scores (or to avoid that button if aiming for low scores). As Schmidt (1969b) described it:

> During a test, the subject sits in front of a small panel with four pushbuttons and four corresponding colored lamps. Each of the pushbuttons simultaneously activates a recorder switch and a trigger switch. The recorder switch serves to register which of the buttons has been pressed. The four trigger switches are connected in parallel such that pressing any one of the buttons closes a circuit, in turn triggering the random lighting of one of the four lamps. The system is designed so that on repeated pressing of the buttons the lamps light in random sequence, i.e., each lamp lights with the same average frequency, and there is no correlation between successively lit lamps or between the buttons pushed and the lamps lit. (p. 101)

Random lighting of the lamps was achieved, following the subject's response, by a sophisticated electronic random event generator that used a radioactive source, strontium 90. (See Schmidt [1970b] for a more complete account of the hardware design and methods of statistical evaluation.) The REG was extensively tested in control trials and found not to deviate significantly from chance.

> The sequence of buttons pressed and lamps lit is recorded automatically on paper punch tape. In the research reported here, the two types of test (trying for a high or low number of hits) were recorded in different codes, such that the evaluating computer could distinguish between them. The number of trials made and hits obtained were displayed to the subject by electromechanical reset-counters. These numbers were also registered by nonreset counters, and the readings of all counters were regularly recorded by hand. This record agreed with the results obtained from the paper tape. The equipment was fraud proof, so that one could, in principle, let the subjects work alone. This was done, however, only in a small part of the tests with subject OC in the first experiment and did not increase the scores. In all other tests the writer was present in the same room with the subject. (Schmidt 1969b, p. 103)

Schmidt's first report was based on two experiments. The subjects in this study were preselected on the basis of their performance in the preliminary tests. In the first experiment there were three subjects. All of them attempted to obtain high scores. Together they did 63,066 trials and scored 16,458 hits, which was 691.5 more than mean chance expectation (MCE). The probability that such a result occurred by chance is smaller than 2×10^{-9}.

In the second experiment, two subjects from the first series and one new subject participated. One subject aimed for high scores and another for low scores. The third aimed high in some trials and low in others. The total number of trials was 20,000. Of these, 10,672 were high-aim trials and 9,328 were low-aim. The combined deviation of hits in the desired direction was 401 greater than MCE, which has an associated probability smaller than 10^{-10}.

In the third experiment, Schmidt (1969a) tested six subjects, including two who had participated in the trials just described. The experiment was designed to test primarily for clairvoyance; the targets were digits from a random number table further shuffled by a congruential generator and recorded on paper punch tape. The subjects completed a total of 7,091 high-aim trials and 7,909 low-aim trials, for a grand total of 15,000. The combined deviation of hits in the desired direction was +260 ($p = 0.3 \times 10^{-6}$).

3.2. Criticisms of Schmidt's REG experiments

Hansel (1980) discussed the "weaknesses" in Schmidt's experiments under three headings: (1) experimental design, (2) unsatisfactory features of the machine, and (3) inability to confirm the findings. He criticized the experimental design (a) for its failure to specify in advance the "exact numbers and types of trials to be undertaken by each subject," (b) for its introduction of high-aim and low-aim conditions, and (c) for its lack of control of the experimenter.

Strictly speaking, criticism (a) is not relevant to the main purpose of the experiment, which was to determine not whether a given subject had ESP, but whether the experiment as a whole provided evidence for ESP. It is true, however, that in Schmidt's first experiment the number of *total* trials was also not specified precisely in advance. The high level of statistical significance obtained, however, renders the possibility that this factor could account for the results extremely unlikely. And, as Hansel acknowledges, this problem was corrected in the later experiments.

Criticism (b) is not substantiated. Noting that high-aim scores gave a positive deviation and low-aim scores a negative deviation, Hansel argued, "The fact that when positive and negative deviations are combined (maintaining their sign) they invariably give a purely chance score suggests that sampling from a common distribution may have taken place" (p. 230). In the first place, this argument fails to account for Experiment I, which involved only the high-aim condition and gave results that were just as significant as in the other experiments. Second, it is not clear how Hansel's criticism could apply to the other experiments, since the high and low conditions were assigned in advance and recorded automatically on paper punch tape *in different codes*. It would seem, in fact, that the introduction of high/low conditions has a certain additional merit in that one condition could be considered as a control for the other, as well as for machine bias. It is of interest that in discussing a different Schmidt experiment, Hansel (1981) himself criticized Schmidt for not having a control condition and recommended the introduction of a condition in which "the subject would not be 'willing' the light to move, or *he would aim at moving the light in the opposite direction*" (p. 32, our italics).

Hansel went on to contend that two different machines, one for high aim and the other for low aim, should have been used. But would not such a procedure have been criticized on the grounds that any obtained difference between the scores could have been due to the opposite bias of the two machines?

Criticism (c) is valid if by "control of the experimenter" Hansel meant control against experimenter fraud. It would have been entirely possible for Schmidt to fake the results if he had wished to. In the extreme case, for example, the whole experimental report could simply have been fabricated. We cannot conceive, however, how a nonintentional error on the part of the experimenter could have artifactually produced the significant results.

Hansel's criticism (2) of the machine itself overlaps criticism (1-b) above and was discussed under that heading.

The final reason given by Hansel for his rejection of Schmidt's results was that they have not been confirmed. But this again seems erroneous, as will be shown in Section 4.1.1 below. Hansel made no mention of several experimental reports already in the literature that did in fact claim to confirm Schmidt's results; he instead referred only to the 1963 report of Smith et al., which gave null results when VERITAC was used to test for ESP. But even this comparison is problematic. First, the machines, experimental procedures, and manipulation of the psychological conditions differed markedly between the two studies. Second, Schmidt's subjects were carefully screened through pretesting procedures, whereas those who participated in the VERITAC experiment were not.

In a more recent publication, Hansel (1981) proposed a scenario that permits the *possibility* of trickery without providing any evidence that fraud had indeed occurred. Referring to one of Schmidt's experiments testing PK (Schmidt 1970a), he claimed that the subject could have shorted "either the +1 or the −1 input in the display panel to the earth line according to whether he wished to produce a high or a low score" (p. 30), which would account for the significant results. This argument seems fallacious. Because the REG and electronic counters were sealed in a metal box and the REG outputs were completely buffered, there was no way the subject could have tampered with the apparatus in the way Hansel suggests. Second, the data were independently recorded on punch tape. Had the subject shorted the tape machine, the total number of punches would have differed from the 128 specified for each run. Inspection of the tapes revealed no such discrepancies (Schmidt, personal communication).

Hansel went on to argue that the experimenter himself could have easily affected the punched record. This is debatable, but the possibility that Schmidt could have faked his data *somehow* has already been acknowledged. Recently, however, Schmidt has published a PK experiment designed to rule out the possibility of his (or his two co-experimenters) falsifying the data without collaboration from at least one of the others (Schmidt et al. 1986). Briefly, Schmidt, located at his lab in San Antonio, Texas, prepared lists of paired six-digit random numbers, called seed numbers, which were to be used to generate sequences of quasirandom binary digits by means of a complex mathematical algorithm known only to Schmidt. These seed numbers were mailed to the private address of Professor Luther Rudolph (L. R.) of Syracuse University. Robert Morris (R. M.) of the same university independently obtained a list of random target directions (high and low), one for each binary sequence, by using his laboratory's own REG. R. M. and L. R. exchanged their copies of the target-direction sequences and the seed numbers and then made the former available to Schmidt.

For the test proper, the subject in San Antonio entered the seed numbers into a computer. The computer then derived the binary sequences, which in turn governed the display on a computer screen of a pendulum swinging with random amplitude. The subject's task was to will the pendulum to swing with large amplitude on high-aim trials and with small amplitude on low-aim trials.[1] At the end of the run, which lasted for about a minute, the display showed the average swing over the run; thus the subject was given feedback about his rate of success.

Schmidt et al. reported significant results in support of their hypothesis. The combined Z for all the ten sessions was 2.71 ($p < .005$). Because (a) the seed numbers for the binary sequences and (b) the target directions were independently derived by Schmidt and Morris, respectively, we know of no way Schmidt or Morris alone could have artifactually obtained the results. Such security procedures involving experimenters working independently in two different laboratories are seldom used in scientific research; but it is understandable that Schmidt felt that the validity of his results should not be based ultimately on his honesty alone.

Of course, the possibility of fraud is still not eliminated completely in this experiment. Even if we grant that Schmidt alone could not have faked the results, it remains possible, though less probable, that Schmidt and Morris, or Morris and Rudolph, could have conspired to produce them spuriously. Perhaps the logical next step is to have a critic participate as a co-experimenter, using the design of Schmidt et al. We would be curious to see how critics would react if such an experiment succeeded.

Hansel's criticisms of Schmidt's experiments are routinely taken as valid by most writers skeptical of psi (e.g., Alcock 1981). One of the few critics of psi who questions the basic premises of Hansel's reasoning on this point is Hyman (1981). "There is no such thing as an experiment immune from trickery," says Hyman. "Even if one assembles all the world's magicians and scientists and puts them to the task of designing a fraud-proof experiment, it cannot be done" (p. 39). Hyman, however, agrees with Hansel that Schmidt's PK experiments "do not provide an adequate case for the existence of psi" (p. 34). His principal reasons are twofold: (1) "Experience shows that the most promising research programs in parapsychology will most likely be passé within a generation or two" (p. 37); and (2) although Schmidt's randomization tests control against "long-term, or even temporary" machine bias, they do not "control against possible short-run biases in the generator output" (p. 38). He suggested, as did Hansel, that matched experimental and control sequences would have been a superior procedure.

The first point is not really a substantive criticism but merely counsels patience. The same thing can be said of research in some other areas of psychology. Moreover, "passé" does not necessarily mean "discredited," and much of the older research in parapsychology has withstood criticism rather well. The second point, as Hyman himself recognizes, "does not automatically provide an alternative explanation for how Schmidt obtained his results" (p. 38). Schmidt, who was aware of such a possibility, notes that "many more randomness tests were done than published to satisfy my own questions about the possibility of temporary random generator malfunctions" (Schmidt 1981, p. 41). Also, it is difficult to see how such malfunctions could account for subjects' ability to anticipate the timing and direction of the hypothesized short-run biases in Schmidt's early PK research, which used a high-aim, low-aim protocol (Schmidt 1970a). Finally, in some of Schmidt's more recent work, direct comparisons *were* made between experimental and control sequences (e.g., Schmidt 1976).

4. The question of replication

Even assuming that it was possible to determine conclusively the proper interpretation of a single experimental result, such an exercise would have little value in the context of doing science. The way the scientist functions is different from the way the historian does, for example. Unique events and isolated facts, unless they lead to, or are capable of leading to, some kind of general law, ordinarily hold little interest for science. Unlike historical facts, most phenomena of science are capable of being repeated. The Battle of Gettysburg will not be fought again. But psi as a laboratory effect must be reasonably capable of being observed repeatedly if one is to study it effectively and to understand it. Thus, as even Hansel (1980) concedes at one point, the importance of a foolproof experiment recedes into the background as the phenomena become increasingly replicable.

Replicability does not necessarily mean that a finding must be reproducible on demand. It is not strictly an either–or situation, but a continuum (Rao 1981b). In this sense of statistical replication, an experiment or an effect may be considered replicated if a series of replication attempts provides statistically significant evidence for the original effect when analyzed as a series.

It may be argued that statistical replication is simply imperfect replication, and that a real phenomenon is something that is *in principle* repeatable. If a phenomenon has occurred once, it will occur again, provided the same set of circumstances arises. If one had perfect understanding of the critical variables, one could invariably predict its occurrence; if one had control over those variables one could produce the phenomenon on demand. The problem is that, in practice, perfect duplication of conditions is impossible to achieve. This is especially true in behavioral science experiments, where the causes of an effect are likely to be complex and difficult to pin down.

This does not mean that replicability cannot be improved substantially if some understanding of these crucial variables can be achieved. Indeed, such understanding is a major goal of scientific investigation. The other side of the coin, however, is that inquiry in such cases begins without this understanding. It is therefore inappropriate to demand absolute or even strong replicability of a phenomenon simply as a prerequisite for further research.

4.1. Examples of replicability in parapsychology

Once we give up the notion of absolute replication, we can see that parapsychological phenomena are replicated in a significant statistical sense. For example, Palmer's (1971) review of so-called sheep–goat studies reveals that in 13 of the 17 experiments that used standard methods of analysis, the "sheep" (the subjects who believed in the possibility of ESP) obtained higher scores than did the "goats" (the subjects who did not believe in ESP), with 6 of the 13 achieving statistical significance. Carl Sargent's

(1981) review of the reports published in English on the association between ESP and extraversion suggests that significant confirmations of a positive relationship occur at over six times the chance rate. However, the most extensive evidence for the statistical replicability of psi comes from the three data bases to be discussed in more detail below.

4.1.1. REGs and psi. Since the publication of the REG results discussed in Section 3.1 above, Schmidt has carried out several other successful REG experiments, mostly involving PK. More to the point, a number of other experimenters have successfully used the same devices or similar ones to test for psi.

The most prominent of these replications comes from the laboratory of Robert Jahn at Princeton University (Jahn 1982; Nelson et al. 1984). Jahn and colleagues use an REG based on a commercial electronic noise source. The hits are counted and displayed on the instrument panel and are permanently recorded on a strip printer as well as a computer. The subject's task is to influence the device mentally to produce an excess of hits on predesignated PK+ trials and an excess of misses on PK− trials. In a total of 195,100 PK+ trials, 22 subjects obtained a mean score of 100.043 (MCE = 100). The mean for the same number of PK− trials was 99.965. Although small in magnitude, both these means are significantly different from mean chance expectation. The combined probability of the results is approximately 3×10^{-4}.

Each trial in Jahn's experiments incorporated alternate positive and negative counting on successive samples to provide an on-line internal control against any systematic bias in the noise source (i.e., positive and negative noise pulses alternated as hits). Also, baseline trials were recorded "under a variety of conditions before, during, and after the active PK trials" (Jahn 1982, p. 148) in a manner resembling that recommended by critics. The mean score for these 179,250 baseline trials was 100.005, which does not differ significantly from chance.

Radin et al. (1985) conducted a preliminary survey of all binary (two-choice) REG experiments published from 1969 (the year of Schmidt's first published REG experiment) to 1984. The sources sampled were the five major refereed parapsychological journals, the bound *Proceedings* of refereed papers presented at the annual Parapsychological Association Conventions, and a report of the Princeton data by Nelson et al. (1984), cited above. The reviewers defined an "experiment" as the "largest possible accumulation of data compatible with a single 'direction of effort' assigned to the subjects" (p. 205). In other words, data from all trials in which subjects aimed for the same binary outcome were pooled, ignoring other experimental conditions or classifications that may have pertained.

The reviewers uncovered 56 reports from approximately 30 principal investigators describing a total of 332 individual experiments. For 30 of the nonsignificant experiments, the authors of the reports provided insufficient data to allow the outcome (deviation of the hit total from chance) to be expressed quantitatively. In each of these cases, the reviewers randomly selected a Z-score from a normal (null) distribution of Z-scores to represent the outcome.

Seventy-one of the 332 experiments (21%) yielded results significant at or beyond the 5% level (2-tailed), and the combined binomial probability for all the studies was 5.4×10^{-43}. The outcome was still significant, although more modestly so, when the data from Schmidt and the Princeton group were removed ($p < 4.25 \times 10^{-7}$).

4.1.2. Ganzfeld and ESP. A second major research paradigm in which the replication rate over a relatively large number of studies has been systematically evaluated concerns ESP in the ganzfeld. The ganzfeld is a homogeneous visual field produced, for example, by placing a halved Ping-Pong ball over each eye with cotton filling around the edges. While the subject relaxes in a comfortable chair or bed, a uniform white or red light is focused on his face from about two feet. Sometimes the subject also listens to "pink" noise through attached earphones. Subjects typically report a pleasant sensation of being immersed in a "sea of light" (Honorton 1977, p. 459).

In a typical ganzfeld-ESP trial, the subject receives approximately 30 minutes of ganzfeld stimulation. After a period of adjustment and relaxation, the subject is asked to report all images, impressions, and so on, that occur at the time. From another room, an experimenter blind to the target monitors the subject's mentation via a microphone link and a one-way mirror. In a room located some distance from the subject, another experimenter acts as the agent. Some time after the subject has been in the ganzfeld, the agent–experimenter opens an envelope containing a target picture (randomly chosen from a pool of four), views it for about 15 minutes, and then stays in the room for an additional 10 minutes. After the completion of the ganzfeld period, the first experimenter gives the subject four pictures and asks him to assign them ranks of 1 through 4 for their correspondence to his mentation. At this time neither the subject nor the first experimenter knows which of the four pictures is the target. The agent–experimenter is then called in and reveals the target picture.

The first ganzfeld experiment in parapsychology was reported by Honorton and Harper (1974). The results of this experiment were subsequently replicated by Terry and Honorton (1976), Braud et al. (1975), and Sargent (1980), among others. According to a recent count adopted both by Honorton (1985) and critic Ray Hyman (1985b), there are 42 published ESP experiments that have used the ganzfeld procedure. After correcting for multiple analyses, if any, Honorton concluded that 19 of the experiments (45%) gave significant evidence for psi at or beyond the 5% level. Moreover, 26 of the 36 studies for which the direction of the effect could be clearly determined (72%) gave deviations in the positive direction, as compared to the 50% expected by chance. Hyman (1985b) dissented, concluding that the "rate of 'successful' replication is probably very close to what should be expected by chance given the various options for multiple testing exhibited in the data base" (p. 25). Later, however, he came to agree with Honorton that "there is an overall significant effect in this data base which cannot reasonably be explained by selective reporting or multiple analysis" (Hyman & Honorton 1986).

4.1.3. The differential effect. Another area of psi research with a large number of studies spanning a long period of time is the one dealing with the differential effect. This is the tendency of individual subjects to score differentially in successive ESP tests when these consist of two contrasting conditions, such as two different sets of targets or two different modes of response. In other words, subjects score above chance in one condition and below chance in the other. The first author's (K.R.R.'s) initial encounter with differential scoring occurred when he attempted to test subjects using both ESP cards and cards consisting of symbols to which the subjects were emotionally attached. In the first experiment, he found not only that the subjects obtained more hits than expected by chance with the cards of their chosen symbols, but also that their scores on cards with ESP symbols were lower than MCE. The scoring pattern with one set of cards was the mirror image of the pattern with the other (Rao 1962). Since then Rao has carried out a large number of tests under a variety of conditions and has found a rather consistent tendency on the part of subjects to show a bimodal response pattern when the ESP test consists of two contrasting conditions (Rao 1965).

It is interesting to note that evidence for the differential effect can be found in a number of studies carried out before and after Rao's studies, even when the experimenters themselves were not looking for it. For example, Rao and Krishna (in press) examined 72 independent comparisons between ESP scores obtained by the same subjects responding to two different classes of targets where interactions with other variables had not been predicted. Their sources were the five major refereed parapsychological journals and reports of refereed papers presented at Parapsychological Association conventions. They found that 45 of the 72 comparisons (63%) showed differential scoring, where we would expect 36 (50%) by chance ($p < .05$). In 19 of the experiments (26%), the scoring rate between the two conditions was significantly different at or beyond the .05 level, though one would expect only 3.6 experiments (5%) to show significant differences by chance.

The meaning of the differential effect is not yet clear. It was not derived from a theory or model and provides no explanatory construct that might help us to understand psi. Rather, it reflects a characteristic of psi in a certain type of design, a characteristic that any adequate theory of psi must ultimately account for. One may call it a descriptive construct as distinct from an explanatory construct. Descriptive constructs are important in the early stages of scientific inquiry because, by defining what it is that a theory must explain, they serve to channel the process of theory development. Much of the research in modern parapsychology is directed toward identifying such descriptive constructs or "effects," with the objective of bringing closer to attainment the ultimate goal of a credible theory of psi.

4.1.4. Overview. The proportions of statistically significant studies in the three areas we have reviewed are as follows: REGs (21%); ganzfeld (45%); differential effect (26%). Given the expected success rate of 5%, these values are not trivial, and they compare favorably with comparable examples from psychology, such as the placebo effect (Moerman 1981) and the experimenter expectancy effect (Rosenthal & Rubin 1978). The latter authors, for example, reviewed evidence on the experimenter expectancy effect in eight types of experiments. The median replication rate was 39%. Except for one highly replicable topic (animal learning: 73%), the percentages ranged from 22% to 44%, which is very similar to what we find in parapsychology.

4.2. Some criticisms

A number of objections can be raised to the kind of procedure we have used in obtaining these replication rates, objections similar to those that have been raised in discussing experimenter expectancy effects (Barber 1969; 1973). Some of these objections will now be discussed in relation to the data under consideration.

4.2.1. Comparability of studies. One objection to such analyses is that the studies included are often not directly comparable. This objection has merit, but only to a point. We should not insist, for example, that all experiments be strict replications of one another. So long as they constitute conceptual replications, methodological differences can often be treated as random variables that actually serve to increase the generality of any conclusions that might be drawn from the analysis. On the other hand, it is usually desirable that the outcomes of the studies be represented by, or reduced to, some common metric. One of Hyman's (1985b) criticisms of the ganzfeld data base, for example, was that the studies used divergent and sometimes multiple measures of the dependent variable, and that the primary measure was sometimes not specified in advance. In response to this objection, Honorton (1985) computed a new analysis, using as a single, uniform measure Z-scores representing the proportion of trials in the experiment in which the subject correctly picked out the target during the judging (i.e., direct hits). This was the measure used in the original ganzfeld experiment by Honorton and Harper (1974), and it was the measure most frequently reported in the data base as a whole. Sufficient information for this analysis was provided for 28 of the 42 experiments in the data base. These experiments came from ten different laboratories. Twenty-three of the 28 experiments (82%) yielded positive Z-scores, 12 of which were individually significant at the .05 level on a one-tailed test. The cumulative Z-score for all 28 studies, computed by the Stouffer method (Rosenthal 1984), was 6.60 ($p < 10^{-9}$).

Both Radin et al. (1985) and Rao and Krishna (in press) dealt with the uniformity issue in their analyses of the REG and differential effect experiments (discussed above) by using as a common metric Z-statistics. In the former case, these represented the proportion of trials that were hits; in the latter case, they represented the difference between the proportions of hits in the two conditions.

4.2.2. Publication bias. A second criticism concerns whether these analyses may suffer from biased selection and so-called publication artifact; that is, nonsignificant results may systematically go unreported, and therefore our sample of studies may not reflect the true state of affairs. Close scrutiny of the field suggests that publication bias cannot explain away the significant number of replications in parapsychology. Parapsychologists are sensitive to the possible impact of unreported negative results, more so than most other scientists. Our professional society, the Parapsychological Association (PA), has advocated a policy of publishing the results of all methodologically sound experiments, irrespective of outcome. Since 1976, this policy has been reflected in the publications of all the journals affiliated with the PA and in the papers accepted for presentation at the annual PA conventions.

This policy, however, cannot guarantee that researchers will submit negative findings for publication. Fortunately, thanks to a technique developed by Rosenthal (1979), we are able to estimate the number of unpublished and nonsignificant experiments that would be necessary to reduce an entire data base to nonsignificance. Honorton (1985), for example, used Rosenthal's technique to estimate that 423 nonsignificant ganzfeld studies would be needed to reduce the direct-hit studies in this data base to a nonsignificant level. Given the complex and time-consuming nature of the ganzfeld procedure, it is unreasonable to suppose that so many experiments exist in the "file drawer." As noted earlier, Hyman now agrees that selective reporting cannot account for the aggregate findings in the ganzfeld data base (Hyman & Honorton 1986).

A particularly ingenious way of estimating the extent of the file-drawer problem was implemented by Radin et al. (1985) in their analysis of the REG data base. By inspecting a graph of the distribution of outcomes, they noted a marked discontinuity at the Z-value associated with statistical significance: There were too many studies at the tail to make a smooth curve. They determined that the curve could be smoothed by adding 95 nonsignificant experiments to the data base. Doing this reduced the combined binomial probability of all the studies from 5.4×10^{-43} to 3.9×10^{-18}, still an impressive value. Using the Stouffer method, Radin et al. then estimated that ten parapsychology laboratories would each have needed to produce nonsignificant studies at the rate of 2.6 per month over the 15 years surveyed to cancel out the effect.

Finally, there are some areas in parapsychology where we can be reasonably certain we have access to all the experiments done. One such area concerns the relationship between ESP performance and the ratings obtained on the Defense Mechanism Test (DMT) developed in Sweden by Ulf Kragh and associates (Kragh & Smith 1970). Because the administration and scoring of this test requires specialized training available to only a few individuals, it has been possible for Dr. Martin Johnson of the University of Utrecht, the leading authority on the DMT and a man very sensitive to the issue of publication bias, to keep track of the number of relevant experiments conducted by qualified persons. In all ten of these studies the less defensive subjects scored higher on the ESP test. In seven of them, this effect was significant at the .05 level, one-tailed (Johnson & Haraldsson 1984).

4.2.3. Controls and flaws. A third line of criticism relates to experimental controls. It is argued, for example, that the replication of an experimental result by other experimenters "does not assure that experimental artifacts were not responsible for the results in the replication as well as in the original experiment" (Alcock 1981, p. 134).

It is true, of course, that the replication of an effect implies nothing directly about its cause. But it is also a basic premise of experimental science that replication reduces the probability of *some* causal explanations, particularly those related to the honesty or competence of individual experimenters. As Alcock (1981) himself states in another context, "It is not enough for a researcher to report his observations with regard to a phenomenon; he could be mistaken, or even dishonest. But if other people, using his methodology, can independently produce the same results, it is much more likely that error and dishonesty are not responsible for them" (p. 133).

A more specific set of criticisms has been offered by Hyman (1985b) with reference to the ganzfeld–ESP data base. He concluded that the case for replication in this area is unconvincing because of the presence of methodoligical flaws such as potential sensory cues (e.g., including the target handled by the sender in the set given to the subject for judging), suboptimal randomization of targets (e.g., hand-shuffling), and multiple statistical analyses of the data. Honorton (1985) replied that Hyman made several unsupported assumptions in his analysis and interpretation of the ganzfeld–ESP data, and, in particular, that he often did not assign flaws properly with respect to his own criteria. Honorton presented his own analyses, arguing that the replication rate is not significantly influenced by the presence or absence of potential flaws in these studies. Although continuing to disagree on the seriousness of the "flaws," the reviewers have agreed that "the present data base does not support any firm conclusion about the relationship between 'flaws' and study outcome (Hyman & Honorton 1986). (Flaw analyses have yet to be reported on the REG and differential effect data bases.)

The Hyman–Honorton ganzfeld debate is continuing in the *Journal of Parapsychology*. Whatever its final outcome, the discussion will lead to a more accurate interpretation of the data and better research in the future. In the final analysis, the case for psi cannot be won or lost by arguments over past experiments, but only by systematic and sustained new research that will survive the test of time. Honorton has recently reported continued success using an automated testing protocol that would appear to answer Hyman's methodological objections to the earlier ganzfeld research (Berger & Honorton 1985; Honorton & Schechter 1986).

4.2.4. "Disbelievers" as replicators. Several critics of psi research (Alcock 1981; Kurtz 1981; Moss & Butler 1978) have argued that the replication work must be done by investigators who are unsympathetic to psi, a category that would exclude most (but not all) parapsychologists. Moss and Butler, for example, argue that "replication by a qualified nonsympathetic observer is the only guard against results which may have been contaminated by a conscious or unconscious bias" (p. 1068).

We are now aware of its being common practice in other sciences to disqualify positive results from experiments conducted by researchers who are favorably disposed to the hypothesis they are testing. The personal beliefs of researchers are rarely reported and may often be difficult to determine reliably. We suspect, however, that if such a standard could be applied retrospectively to published research in psychology, for example, there would not be much left. The fact that parapsychologists are singled out for this treatment is symptomatic of the often ad hominem nature of the psi controversy. We have yet to hear a critic suggest that negative results from "disbelievers" in psi be rejected on this basis.

Although it is reasonable to assume that experimenters who obtained strong positive results in the first few psi experiments they conducted were converted to a "belief" in psi by these results (if they were not "believers" already), we have far too few data to draw any conclusions about the distribution of attitudes of investigators at the time they undertook their first psi experiments. Thus we really do not know how many "disbelievers" have obtained positive psi results.

Finally, one cannot assume that confirmatory evidence, even from hardened "disbelievers," will necessarily be acknowledged as such. *BBS* readers might find it instructive in this connection to study what happened when certain members of the Committee for Scientific Investigation of Claims of the Paranormal quite unexpectedly confirmed Michel Gauquelin's astrological "Mars Effect." (See *Zetetic Scholar* 1982a; 1982b; 1983; and references contained therein.)

On the other hand, the fact that the outcomes of psi experiments seem to be sensitive, at least to a degree, to the identity of the experimenter or principal investigator is a legitimate cause for concern. This "experimenter effect" in parapsychology has long been recognized and extensively discussed within the field (e.g., Kennedy, J. E. & Taddonio 1976; White 1976a; 1976b); even some strong proponents of psi have had trouble obtaining positive results in their experiments. The jury is still out as to why this state of affairs exists. Until more is known, it is unwarranted and unfair to jump to the conclusion that the experimenter effect is due to fraud, negligence, or incompetence on the part of the successful experimenters, especially in the absence of supporting empirical evidence. The number of trained scientists who have obtained positive results in psi experiments is by no means inconsiderable, and many of these scientists have published in orthodox areas. More important, other plausible explanations of the experimenter effect can be proposed. For example, it is not implausible from a psychological point of view that an experimenter who does not expect positive results could convey this attitude to his subjects by nonverbal cues, thereby adversely affecting their confidence or motivation and thus their performance on the psi task. There is evidence from psychology for just such a process (Rosenthal & Rubin 1978). In addition, several studies within parapsychology that compared experimenters who had different attitudes or expectations about psi, or who behaved differently toward their subjects, have provided more direct support for this hypothesis (e.g., Honorton et al. 1975; Parker 1975; Taddonio 1976).

The correct explanation(s) of the experimenter effect can come only from more research. This will come sooner if more scientists outside the parapsychological community – "believers," "disbelievers," and neutrals – can be persuaded to undertake psi experiments of their own, and to publish their results irrespective of outcome. Despite our remarks earlier in this section, we think that the involvement of a wider range of investigators in psi research is important and we wish to encourage such involvement. Indeed, that was one of our objectives in writing this *BBS* target article. We and other parapsychologists would be pleased to consult with any qualified scientist who would like to undertake such an experiment.

5. Patterns, order, and sense in parapsychology

Has parapsychology gone any further than merely suggesting that anomalies exist? We think it has. Although some work in the field is still concerned with demonstrating the integrity of the anomalies, emphasis in recent years has shifted strongly to so-called process-oriented research designed to uncover lawful regularities between psi and other psychological or physical variables. For example, there have been successful attempts to relate psi to subjects' beliefs and attitudes (Schmeidler & McConnell 1958), personality and motivation (Eysenck 1967; Honorton & Schechter 1986), and to cognitive variables such as memory (Rao et al. 1977), visual imagery (Kelly et al. 1975), and stereotypy of responses to ESP target sequences (Stanford 1975). We would like to focus here, however, on one hypothesis that appears to bring together a large and diverse body of experimental results: the idea that psi may be facilitated by procedures that result in the reduction of meaningful sensory and proprioceptive input to the organism, and the concomitant redirection of attention to internally generated imagery. This hypothesis is known in parapsychology as the noise reduction model.

Whatever its "real" mechanism, ESP may usefully be thought of as behaving like a weak signal that must compete for the information-processing resources of the organism. It follows that the reduction of ongoing sensorimotor activity may facilitate ESP detection by the organism. As illustrated in a book by the psychologist Harvey Irwin (1979), the noise reduction model fits in well with concepts that are widely accepted in cognitive psychology and information-processing theory. The model is particularly relevant to the notion of limits in the information-processing capacity of the organism (Kahneman 1973); namely, the more internal and external "noise" the system must process, the less is available to process possible psi information.

It is interesting that most of the traditional techniques of "psychic" development seem to involve some form of reduced vigilance or "noise reduction." For example, the practice of yoga, which is believed among other things to help develop ESP ability, appears to involve procedures that control habitual sensory, autonomic, and cognitive processes (Rao et al. 1978). The first five of the eight stages in Patanjali's yoga, for example, are preparatory and are aimed at achieving voluntary control of internal processes. The ability of yogins to exercise unusual control over heartbeat and EEG activity, to cause sweat on certain parts of the body, and become physiologically nonresponsive to external stimuli has been satisfactorily documented (Anand et al. 1961; Wallace 1970; Wallace et al. 1971). The final three stages of yoga are *dharana* (concentration), *dhyana* (meditation), and *samadhi* (a state of stillness of the mind). If the introspective accounts of the yogins are any guide, the *dharana* state seems to involve intense focusing of attention on a single object, whereas meditation (*dhyana*) enables the practitioner to hold that focus over an extended period of time, which is believed to result in a stand-still state of mind (*samadhi*).

This state is also described as an expansion of consciousness that goes beyond the object of perceptual attention (Dasgupta 1930). There is voluminous phenomenological information on this, along with a modicum of physiological data (see, e.g., Das & Gastaut 1955).

Historically, many of those who have claimed successful psi receptivity have also claimed that they did their best when they were physically relaxed and when the mind was in a "blank" state. Rhea White (1964), who reviewed the early literature on this topic, concluded that attempts "to still the body and mind" are common among the techniques used by successful psi subjects. Mary Sinclair, whom her husband, Upton Sinclair, found to be an excellent psi subject, recommended for a successful psi outcome that "you first give yourself a 'suggestion' to the effect that you will relax your mind and your body, making the body insensitive and the mind a blank" (Sinclair 1930, p. 180). White (1964) further elaborated this technique and classified it into four stages: (1) relaxation; (2) engaging the conscious mind by keeping it blank or focusing on a single mental image or feeling, perhaps following this by a "demand" that the psychic impression come; (3) waiting patiently for the impression to appear; and (4) assessing rationally if the impression is psychic.

There is also a large body of experimental evidence that procedures enabling a subject to limit extraneous sensory and proprioceptive input are conducive to the manifestation of psi. Much of this evidence has been comprehensively reviewed by Honorton (1977), so we will limit ourselves to a brief discussion of work in five areas – ganzfeld stimulation, hypnosis, relaxation, meditation, and dreams.

5.1. Ganzfeld and ESP

The research on ESP in the ganzfeld has already been discussed at some length. One additional point may be added that is particularly relevant to the present discussion: Those studies that assessed the self-reported effects of the ganzfeld on subjects' state of consciousness have generally found that the largest mean deviation scores from chance on the ESP test occurred among those subjects who claimed the greatest psychological effect from the manipulation (Palmer 1978; Sargent 1980).

5.2. Hypnosis and ESP

There is an extensive experimental literature on ESP and hypnosis. Fahler and Cadoret (1958), for example, tested college students in two formal experiments using a clairvoyance type of card-guessing task. In half of the trials the subjects were "under hypnosis" as they attempted to guess ESP cards screened from their view, and in the other half they guessed the targets while in a waking state. The order of testing was counterbalanced. In both experiments the subjects did significantly better in the hypnotic condition than in the waking condition.

In a careful review, Ephraim Schechter (1984) evaluated data from 25 experiments in which ESP performance was compared in hypnotic and control conditions. The results of 5 of these experiments are uninterpretable for a variety of reasons. Of the remaining 20 studies, 16 show higher scores for the hypnotic condition, with 7 of them showing statistical significance. None of the four reversals are significant.

5.3. Relaxation and ESP

The most extensive work in this area has been carried out by William Braud. In one of the best designed of these studies (Braud & Braud 1974), 20 volunteer subjects were assigned randomly to "relaxation" or "tension" conditions. Those in the relaxation condition went through a taped, progressive-relaxation procedure (an adaptation of Jacobson's) before taking an ESP test, which was to guess the picture being "transmitted" by an agent in another room. The subjects in the other group were given taped, tension-inducing instructions before they did the same ESP test. Each subject's level of physical tension was assessed through electromyographic recordings and self-ratings. Both measures revealed a significant decrease in tension among the subjects in the relaxation group and a significant increase among those in the tension group; as predicted, the ESP scores of the subjects in the relaxation group were significantly above chance and significantly higher than those of the subjects in the tension group.

Although no formal meta-analyses have been conducted on this data base, our own informal survey uncovered 13 series from six researchers that have reported significant effects (two-tailed) favoring the facilitative effect of relaxation, and only one significant reversal using the same criteria.

5.4. Meditation and ESP

Studies investigating meditation and psi suggest a positive relationship between these two variables. Rao et al. (1978) reported three series of experiments with a total of 59 subjects who had various degrees of proficiency in yoga and meditation. The subjects were given two ESP tests both before and after they meditated for at least half an hour. In one test the subjects "blind matched" cards with ESP symbols against target cards concealed in opaque black envelopes, and in the other test they attempted to describe concealed pictures. The results of both tests yielded independently significant premeditation-to-postmeditation differences when the three series were pooled. The card-testing results were also significant for each of the three series separately.

Again, no formal meta-analyses have been conducted on this data base. However, our own informal survey uncovered 12 series from six researchers that have reported significant effects (two-tailed) favoring the facilitative effect of meditation, and only one significant reversal, using the same criteria.

5.5. ESP in dreams

Finally, mention should be made of a successful series of experiments on ESP in dreams conducted at Maimonides Medical Center (Ullman et al. 1973). In a typical experiment, a sender attempted to transmit the content of a randomly selected art print to a subject sleeping in an isolated room. When physiological monitoring indicated that the subject was dreaming, an experimenter blind to the target awakened the subject and elicited a dream report. The following morning, a tape of the dream reports was played back to the subject, who added associational material and a "guess for the night." Subsequently, outside judges and/or the subject attempted to match the randomly ordered targets and dream transcripts from a series of sessions on a blind basis.

In an article that appeared recently in *American Psychologist,* Irvin Child (1985) reviewed 15 separate series from the Maimonides program. After eliminating data from analyses that may have been compromised by nonindependence of the judgings, he concluded that the remaining data were collectively significant both for the independent judges and for the subjects as judges. Child's article also documents several instances of gross misrepresentation of the Maimonides experiments in commentaries by critics.

In contrast to the other research considered in this section, there have been no independent replications of the Maimonides research that have provided significant results. Two major failures to replicate have been reported (Belvedere & Foulkes 1971; Foulkes et al. 1972), and one other is equivocal (Globus et al. 1968).

5.6. *Some criticisms*

Considering the legendary elusiveness of psi, the rate of reported success in the psi studies involving sensory noise reduction, although far from perfect, is impressive, even more so because the results appear to make sense in the context of both traditional psychic training practices and theories from orthodox psychology. One could of course point out that studies such as the so-called remote-viewing experiments (Targ & Puthoff 1977), which do not involve any explicit procedures for reducing sensory noise, have also recorded success rates of about 50%, arguing that our rationale is unsupported by these studies. However, such an argument does not take into account the fact that most of the successful remote viewing experiments, unlike the experiments discussed above, used subjects that were preselected for psychic talent and thus less likely than ordinary volunteers to need a supportive cognitive state to perform successfully. Second, there is reason to believe that at least some of these subjects attempted to reduce noise on their own. Marilyn Schlitz, a highly successful remote viewing subject, put herself in a "calm state throughout," even though she used no formal relaxation procedure (Schlitz & Gruber 1980). Dunne and Bisaha (1978) asked their remote viewing subjects to "relax and clear their minds" prior to the remote viewing test.

Even if one were to concede that successful remote viewers are generally in an ordinary state of consciousness during the psi task, it does not follow that they might not have performed even better had they been in an altered state of the type we have been discussing. This observation, however, brings to light another criticism of the studies supporting the noise reduction model. Many of these studies, in particular most of the ganzfeld and relaxation experiments, failed to use control groups or other means of assessing whether the induction procedure was actually responsible for the positive scoring. Among those studies that did use such controls, the designs still did always preclude other interpretations of the results (see, e.g., Stanford 1987). Especially in the experiments using within-subjects designs, relative success in the experimental condition might sometimes be attributable to expectancy effects or demand characteristics.

More research will be needed before the status of the noise reduction model can be conclusively determined. A large body of empirical data from diverse sources is nevertheless consistent with this hypothesis. This fact is sufficient to support the more modest point we are trying to make: Psi data fall into patterns that make psychological sense and encourage a systematic program of research.

6. Practical significance

The remaining criticism that needs to be addressed concerns practical significance. Even if one concedes that the preceding criticisms have been addressed satisfactorily, it can be argued that the results of psi experiments are trivial and of no practical or clinical importance. It is certainly true that the effect sizes in most psi experiments are small. For example, the effects reported by Schmidt in his REG experiments rarely exceed chance expectation by more than a few percent. Such outcomes hardly seem to be practically useful.

There are fallacies in this line of criticism, however. First, it fails to acknowledge the distinction between basic and applied research. Practical significance is indeed important if the objective is to determine whether a process can be applied to solve "real-world" problems. Parapsychology, however, is devoted almost exclusively to basic research, where the objective is to address theoretical issues. Psi results seem to violate expectations derived from generally accepted physical theory, and this makes them of theoretical interest irrespective of their magnitude. For example, many of the most important experiments in physics deal with effects of very small magnitude.

The above criticism is problematic even from the applied perspective, however, because techniques from information theory can be implemented to amplify a weak effect of the type commonly found in psi experiments. In one experiment, for example, Ryzl (1966) had the subject Stepanek guess whether the green or white sides of 30 cards placed inside opaque envelopes were uppermost. The cards were rerandomized and Stepanek guessed the order again. This process was repeated until Stepanek's distribution of guesses on each of 10 principal cards favored either green or white to a prespecified degree. Other criteria involving the other 20 cards also had to be met. The result was a single "majority vote" on each of the 10 principal cards. In each of five experiments, Stepanek's majority votes duplicated the target order of the 10 principal cards perfectly (100%), although his success rate on individual guesses was only 62%. Other examples of this approach have also been documented (e.g., Carpenter 1975; Puthoff 1985).

The reason that psi has not yet been applied on a broad scale has to do not with the size of the effects but with their unreliability, which (as discussed above) probably reflects our lack of understanding of the factors that affect performance on psi tasks. Uncovering these factors is a prime objective of modern parapsychological research.

If psi anomalies do in fact turn out to represent some heretofore unrecognized and far-reaching ability to acquire information and manipulate the environment, and if this ability could be brought under conscious control, the practical applications and potential benefits to mankind seem almost limitless. It is easy to put parapsychologists on the defensive by citing the slow progress that has been made to date in coming to grips with the anomalies. What such an approach overlooks is the importance of solving the admittedly unsolved puzzle that the anomalies represent. It seems to us that too many commentators on both sides of the psi controversy place excessive faith in what amounts to little more than speculations about the true nature of the anomalies. Only by continued research, preferably supported in a meaningful way by the scientific community at large, will the speculations turn into knowledge.

7. Conclusion

We find that the frequency of replications, especially with regard to the noise reduction hypothesis, indicates that we are indeed on the trail of something interesting. At the same time, we cannot totally rule out the possibility that we may yet discover a hidden artifact or set of artifacts that would provide a satisfactory conventional explanation of the results (and which might, in their own way, likewise prove interesting). Such an open approach, which is widely shared within the parapsychological community (Parapsychological Association 1986), is dictated by the anomalous nature of psi and the fact that there is still no verified theory of the mechanism(s) involved in psi interactions. Scientists working in this area must accordingly approach *all* hypotheses with an attitude of skepticism and must show a readiness to look at various alternatives (Palmer 1986a). Critics with a great deal of a priori skepticism about psi have reasonable grounds for not accepting omegic hypotheses – that is, that the anomalies represent a new principle of nature. At the same time, they have little justification for choosing to close their minds to the alternative possibility – namely, that the anomalies might reveal a currently unrecognized human capacity of great potential importance. If they do close their minds, they make the same mistake as those "believers in the paranormal" who refuse to study evidence and arguments contrary to their beliefs.

At the least, there is now an excellent prima facie case for the statistical repeatability of the anomalies under certain conditions. There appears to be a common thread running through these studies, diverse though they may be, in the techniques of eliciting and measuring psi. This commonality appears, at least in a crude and preliminary way, to make some theoretical sense and is leading to work now in progress at various laboratories to refine and consolidate the methods and concepts.

We have discussed here some experimental evidence for the reality of psi, as well as the objections of critics to such evidence. We have also considered the idea that sensory noise reduction may be favorable to psi, sketching the experimental results that bear on this hypothesis. The following conclusions seem to emerge:

(1) Schmidt's results and many other parapsychological findings would be taken seriously if they related to a conventional area in science, for standard methodological and statistical criticisms have been answered.

(2) No single experiment, no matter how carefully designed and executed, can be expected to settle a controversial claim. The results of one good experiment do no more than make a claim. The significance of that claim is proportional to the degree that experiments supporting it are successfully replicated, and the degree of research and hypothesis-testing it generates. Also important is its potential for contributing to a theoretical understanding of the natural world and for practical application.

(3) The issue of replication and the meaning of experimental results in psi research have been a primary concern of parapsychologists. The discussion of the studies bearing on psi and sensory noise reduction and the rationale behind them show (a) a moderately significant rate of replication (in a statistical sense) and (b) the possibility of finding conditions that favor or inhibit psi. Together, these studies make a strong prima facie case for a genuine scientific anomaly and provide a viable research program.

(4) Further clarity and precision in the concepts and hypotheses are needed. Noise reduction, for example, needs to be defined more precisely. Some improvements in experimental design may have to be introduced to deal with the central issue of how psi operates. No mechanism or theory that would adequately explain psi has been validated. Those who accord an extremely low subjective probability to omegic hypotheses may therefore justifiably demand more and better evidence. But demanding such evidence is not the same as questioning the credibility of past research.

(5) The final settlement of the question of the status of psi will have to depend on further research. The scientific legitimacy of psi cannot be denied by personal innuendos and ad hominem arguments, just as it cannot be established by preaching. One can only hope that the climate of scientific opinion will be sufficiently tolerant to permit free and open inquiry by those who have the necessary skills and interest.

NOTE

1. The theoretical rationale of the study was that the subject could psychokinetically influence the selection of the random seed numbers retroactively. We will not elaborate this hypothesis further, as it is not directly relevant to the control features of the experiment.

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Parapsychology: Science of the anomalous or search for the soul?

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Abstract: Although there has been over a century of formal empirical inquiry, parapsychologists have clearly failed to produce a single reliable demonstration of "paranormal," or "psi," phenomena. Although many parapsychological research projects have been carried out under what have been described as well-controlled conditions, this does not by itself make a science, for unless and until it can be demonstrated that paranormal phenomena really exist, there is no subject matter around which a science can develop. Indeed, parapsychologists have not even succeeded in developing a reasonable definition of paranormal phenomena that does not involve, or imply, some aspect of mind–body dualism. Moreover, parapsychology has developed several principles (such as the experimenter effect) which can be used to explain away failures, and the use of these principles contributes to making the psi-hypothesis unfalsifiable.

The "anything goes" attitude in parapsychology, which seems to lend credence to virtually any "paranormal" claim, serves to weaken the credibility of parapsychological endeavors in the eyes of critics. This general willingness to suspend doubt is another indication that parapsychology is more than the quest to explain anomalous experiences, as is claimed. It is argued in this paper that parapsychological inquiry reflects the attempt to establish the reality of a nonmaterial aspect of human existence, rather than a search for explanations for anomalous phenomena.

Keywords: anomaly; causality; dualism; ESP; experimental method; explanation; methodology; parapsychology; philosophy of science; replication

0. Introduction

It is curious that, in this age of unprecedented literacy and unceasing scientific and technological progress, many people are prepared to accept that spoons can be bent by the power of the mind alone, that disease can be cured by the laying on of hands, that water can be located by means of a forked willow stick, or that the mind can influence the decay of radioactive substances. It is even more curious when such claims are put forth and defended by people trained in the ways of science.

Most *BBS* readers, I would imagine, have little difficulty dismissing popular occult beliefs in astrology, palmistry, the tarot, or biorhythms. However, those same readers may not be nearly so cavalier about disregarding such supposed "paranormal" (also synonymously referred to as "parapsychological" or "psi") phenomena as extrasensory perception ("ESP") or psychokinesis ("PK"). ESP refers to the supposed ability to obtain knowledge of a target object or of another person's mental activity in the absence of sensory contact, and PK is the putative ability of the mind to influence matter directly. Belief in such phenomena is actually very widespread, not only among members of the general public but also among university students (e.g., Alcock 1981; Gray 1984; Otis & Alcock 1982).

Such belief is no doubt tied, at least in part, to the fact that many people, perhaps even most, have from time to time had direct personal experiences that seemed to be "telepathic" or "precognitive" or "psychokinetic." Indeed, a number of surveys (e.g., Alcock 1981; Evans 1973; Irwin 1985a; McConnell 1977; Sheils & Berg 1977) have found personal experience to be the major reason given by respondents for their belief in paranormal phenomena. This is not surprising: Given their often powerful emotional impact, combined with a lack of understanding about the myriad "normal" ways in which these experiences can come about (e.g., see Alcock 1981; Marks & Kammann 1980; Neher 1980; Reed 1972; Zusne & Jones 1982), it is easy to ascribe paranormal explanations to odd experiences that one cannot readily explain otherwise.

"Parapsychology" is defined as the scientific study of paranormal phenomena (Thalbourne 1982). The study of the paranormal was historically associated with the so-called occult sciences such as astrology and numerology; a more direct progenitor was the spiritualism craze of the late nineteenth and early twentieth centuries. However, parapsychology stands well apart from these belief systems in a number of ways:

0.1. Scientific orientation. For over a century, there has been careful and deliberate investigation of psi phenomena by people trained in the methods of science. In the past 50 years, much of this research has been laboratory-based and carried out in university settings. Currently, parapsychological research is being conducted at such prestigious academic institutions as the University of Edinburgh and Princeton University.

Throughout the last century and continuing to the present, a number of very prominent natural and social scientists have been proponents and supporters of parapsychological research (see Hyman 1985a; Rogo 1986), including physicists Sir William Crookes, Lord Rayleigh (Nobel Prize, 1904), Wolfgang Pauli (Nobel Prize, 1945), Brian Josephson (Nobel Prize, 1973), and David Bohm; naturalist Alfred Russell Wallace; chemist Robert Hare; physiologist Charles Richet (Nobel Prize, 1913); psychologists William James, William McDougall, Carl Jung, Sir Cyril Burt, and Hans Eysenck; anthropologist Margaret Mead; mathematician John Taylor (who became convinced of the reality of psi phenomena on the basis of Uri Geller's purported feats [Taylor 1975], only subsequently to repudiate his belief in such phenomena [Taylor & Balanovski 1979]); and Robert Jahn of the Engineering Department at Princeton University.

There has also been a history of professional interaction between conventional science and parapsychology at scientific conferences, through symposia on the paranormal and invited addresses by parapsychologists (e.g., American Association for the Advancement of Science, 1975, 1978, 1984; American Physical Society, 1979; American Psychological Association, 1966, 1967, 1975, 1984, 1985), although admittedly such opportunities for parapsychologists to present their ideas and evidence have been limited.

0.2. Organization. As a research discipline, parapsychology is organized very much the way various disciplines of mainstream science are. There are professional bodies that emphasize empirical enquiry using scientific methodology and that encourage high research standards. (One of these, the Parapsychological Association, over half of whose 300 or so members hold doctorates in science, engineering, or medicine [McConnell 1983], has been affiliated with the American Association for the Advancement of Science since 1969.) Annual research conferences are held. Research grants are awarded. There is substantial empirical literature in the field – including several research journals and many books, some of which have been published by leading scientific publishers (e.g., the *Handbook of parapsychology* [Wolman 1977a], the *Foundations of parapsychology* [Edge et al. 1986], and the series *Advances in parapsychological research* [Krippner 1977; 1978a; 1982a; 1984]).

0.3. Academic involvement. Courses in parapsychology are offered for academic credit at about 50 colleges and universities in the United States (McConnell 1983); a few even grant degrees in the subject (see Stanford 1978). Ph.D.'s have been awarded for parapsychological research at Cambridge University, the University of Edinburgh, Surrey University, Purdue University, the University of the Witwatersrand, and the City University of New York, among others. The University of Edinburgh has recently established the Koestler Chair in Parapsychology, which is endowed by a bequest from the late Arthur Koestler, a long-time supporter of parapsychology.

How do members of academia view claims about psi? In one survey of humanities and science professors at two large universities (University of Michigan and University of Toronto; response rate 53%), only about one-third of the respondents indicated believing in paranormal phenomena (Otis & Alcock 1982); there was no clear difference between representatives of the sciences and the humanities. This is consistent with the results of a smaller survey conducted at two other Canadian universities (Alcock 1981). Yet, Wagner and Monnet (1979), in a much larger survey of professors at 120 colleges and universities in the United States (response rate 49.5%), found that 73% of the respondents from the humanities, arts, and education indicated they believed ESP to be either an established fact or a likely possibility, whereas only 55% of the respondents from the natural sciences and 34% of the psychologists did likewise. Whether the differences between the results of the two surveys reflect differences in the questions asked or differences in the groups sampled (the former study was limited to respondents from two large and prestigious universities) is not clear. (It should be noted in any case that such surveys are always subject to a response bias, in that there is likely to be a differential response rate as a function of attitude toward the subject matter being addressed.)

Although all of this might suggest that parapsychology is a serious and professional research discipline that is viewed with respect within university settings, at best parapsychology struggles to maintain a toe-hold at the fringes of academia; mainstream science continues virtually to ignore its subject matter or even to reject and ridicule it. One finds no mention of psi phenomena in textbooks of physics or chemistry or biology. Lecturers do not address the paranormal in undergraduate or graduate science programs. Psychology students are rarely taught anything about the subject. Parapsychological research papers are only very infrequently published in the journals of "normal" science, and parapsychologists have criticized leading scientific publications such as *Science, The American Journal of Physics*, and *American Psychologist* for suppressing the dissemination of parapsychological research findings (Honorton 1978a; McConnell 1983). Funds for parapsychological research are usually generated within parapsychology itself or come from private donors; the agencies that fund normal science turn a blind, or even hostile, eye toward parapsychological research proposals. The United States government, however, has provided multi-million-dollar support for psi research into remote viewing at SRI International in California [Targ & Harary 1984].)

What accounts for the disparity between what would seem to be a substantial degree of professionalism in parapsychology on the one hand, and the continuing relegation of parapsychology to the fringes of science on the other? For one thing, parapsychology continually encounters opposition from mainstream psychology; psychologists appear to constitute the most skeptical group concerning whether psi is likely to exist (Alcock 1981; Wagner & Monnet 1979). Second, people who may serve as the "gatekeepers" of science, in that they are very influential in determining what is and is not the proper subject matter of science, are skeptical about psi. A recent survey of "elite" scientists (Council members and selected section committee members of the American Association for the Advancement of Science) revealed the highest level of skepticism regarding ESP of any group surveyed in the last 20 years (McClenon 1982): Fewer than 4% of the 339 respondents (the response rate was

71%) viewed ESP as scientifically established. (However, another 25% considered it to be a likely possibility, indicating about the same proportion of favourableness as reported by Otis and Alcock [1982], cited above.) Fifty percent considered ESP to be impossible or a remote possibility.

In McClenon's (1982) view, this negativity is based on the threat that paranormal phenomena, were they to exist, would pose to the prevailing scientific worldview. A rather different viewpoint, which is part of the thesis of this paper, is that parapsychology, over its century or so of existence as an empirical research endeavor, has simply failed to produce evidence worthy of scientific status. Of course, *both* these views could be correct.

To facilitate the discussion of this issue, I shall proceed by posing a number of questions I consider to be important concerning psi and parapsychology:

1. What is psi; how is it defined?
2. Is psi "possible"?
3. If psi exists, how can it be detected?
4. What is the evidence that psi exists?
5. Does parapsychology follow the rules of science?
6. Are the critics fair?
7. Is rapprochement possible between psychology and parapsychology?

Let us consider each of these questions in turn.

1. What is psi?

Although it may at first seem straightforward to define or catalogue paranormal phenomena, it turns out to be a difficult task indeed, for there is a considerable spectrum of opinion even within parapsychology as to which ostensible phenomena are likely to be genuinely paranormal and which are probably based on error and self-delusion. For example, although many parapsychologists might scoff at such claims, some believe that "psychic healers," through the laying on of hands, can speed the healing of wounds and slow the growth of fungi (Krippner 1982b); others believe that some gifted persons can project images onto photographic film (Eisenbud 1977), that water sources, or even lost treasure, can be located by "dowsing" with a willow stick (Bird 1977; Schmeidler 1977), that reincarnation warrants serious investigation (Child 1984; Stevenson 1977), that one's personality can leave and return to the body at will and may even be able to travel through outer space (Targ & Puthoff 1977), and that deathbed visions may be indicative of survival after death (Otis & Haraldsson 1978).

Because there is no general agreement on what psi is, or at least how it manifests itself, parapsychologists have found it easier to define it in terms of what it is *not*. The term "psi" itself was introduced by Thouless (1942) as a neutral label in order to avoid the many associations that terms such as "psychic phenomena" and "extrasensory perception" have developed over the years, and psi is defined simply as "interactions between organisms and their environment (including other organisms) which are *not* mediated by recognized sensorimotor functions" (Krippner 1977, p. 2; my italics). Psi phenomena, then, are explicitly defined in a negative manner: To demonstrate that psi has occurred, one must first eliminate all *normal* sensorimotor explanations.

Although only a few parapsychologists appear to share his conservatism, Palmer (1985a; 1986a) argues that until parapsychologists have produced a positive theory of psi which describes the properties that must be present in order to claim that psi has occurred, all they can claim to have demonstrated is the occurrence of a number of anomalies which themselves constitute the subject matter of psi. Seemingly paranormal events might be explicable in terms of conventional science or science as it will be understood in the future, he says, or, indeed, such events might be due to errors in interpretation or measurement or statistical analysis. He recommends that the term "paranormal phenomena" be supplanted by a much less committed term such as "ostensible psychic events."

Palmer's circumspection is commendable and would find favour with most critics of parapsychology. However, it is rare to find parapsychological research reports or other kinds of literature treating apparent anomalies in such a noncommittal fashion. Most, in fact, treat psi not as a description of an anomaly but as a causative agent.

There is a second and more important sense in which psi is negatively defined, albeit implicitly, and that is in terms of its incompatibility with the prevailing scientific worldview (Boring 1966; Flew 1980; Mackenzie & Mackenzie 1980): In some way or another, psi phenomena, to be considered as such, are impossible if the current worldview is correct. There are two different camps within modern parapsychology regarding this incompatibility (Beloff 1977):

1.1. Incompleteness of current science. Just as the scientific worldview changed to accept the extraterrestrial source of meteorites and the constancy of the speed of light, so too, according to this viewpoint, it must ultimately accommodate psi. Thus, "paranormal" phenomena are part of the natural order, but a part of that order which is not yet understood; as soon as scientific knowledge advances to the point that the paranormal is comprehensible, then the latter will become part of an expanded normal science (Truzzi 1982).

This process has been manifested already in several instances: Bat navigation was taken to involve psi until the echo-sounding apparatus of bats was discovered, at which time it became part of the normal scientific domain and of no further interest to parapsychologists (Boring 1966). Bird navigation (Pratt 1953; 1956) and hypnosis (see McConnell 1983; Spanos 1986) are other examples of phenomena that have passed from the realm of the paranormal to the normal.

1.2. A nonphysical dimension of existence. According to this perspective, paranormal phenomena mark the outer limits of the scientific worldview, and beyond those limits "lies the domain of mind liberated from its dependence on the brain. On this view, parapsychology, using the methods of science, becomes a vindication of the essentially spiritual nature of man which must forever defy strict scientific analysis" (Beloff 1977, p. 21).

Of these two perspectives, the incompleteness approach would no doubt be more acceptable to most scientists. Yet, it does not really capture the *flavour* of the paranormal. Whereas anomaly is, it would seem, a necessary condition for paranormality, it is not a sufficient one.

Were it sufficient, then all anomalies throughout the history of science would have to have been considered "paranormal," whereas it is clear that they have not been considered as such (Braude 1978).

Braude (1978) suggests that a definition of the paranormal must go beyond anomaly to include the notion that it "thwarts our familiar expectations about what sorts of things can happen to the sorts of objects involved" (p. 241). Yet, as Mabbett (1982) points out in response to Braude, experimental parapsychological studies that are taken to demonstrate the reality of psi typically produce scoring rates that are only slightly above chance; these hardly thwart peoples' expectations, and "even the thoughtful layman would be unwilling to regard such results as evidence of anything but luck without a little assurance or instruction from the expert statistician" (p. 340).

On the other hand, the bizarre and paradoxical properties of light, as described by relativity theory, would no doubt have been unexpected by laymen as well as by scientists prior to Einstein, Mabbett says, yet most people would not have regarded these properties as paranormal. Mabbett argues that paranormal phenomena are psychological in the sense that they involve mind or consciousness, whatever these may be, and that they reflect a relationship between the mental and physical worlds that is radically different from that conceived of by science.

What is being struggled with here by Braude and Mabbett is that, more than being simple anomalies, paranormal phenomena have a special and particular relationship to the human mind. Indeed, as I have discussed in greater detail elsewhere (Alcock 1985), it is hard to escape the conclusion that the concept of paranormality implicitly involves *mind–body dualism* (see Wolman 1977b), the idea that mental processes cannot be reduced to physical processes and that the mind, or part of it, is nonphysical in nature.

The late Gardner Murphy (1961), once president of the American Psychological Association and one of parapsychology's most erudite and persuasive proponents, argued that even if the paranormal were to be defined only in terms of anomaly, this would still lead to a dualism of some sort because of its independence from considerations of time and space. Indeed, parapsychologists have at times insisted that psi phenomena are distinguished from the other phenomena of psychology by virtue of the fact that they are of a nonphysical nature (e.g., Rhine, J. B. & Pratt 1957). Although the boldness of such a declaration might well raise the hackles of some modern parapsychologists, most of them do seem to accept such dualism (Thalbourne 1984). The influence of dualistic thinking creates a deep schism between parapsychology and modern science.

In summary, then, although some modern parapsychologists prefer to speak only of anomalies, these anomalies, if they are to be of continuing interest to parapsychology, must ultimately involve some radically different relationship between consciousness and the physical world than that held to be possible by contemporary science. Some parapsychologists might deny being mind–body dualists, but they would do well to consider just how they are going to define their subject matter without some reference to the independence of the mind from the materialistic realm (Rhine, L. E. 1967).

2. Is psi "possible"?

Psi phenomena are defined implicitly in terms of their incompatibility with the contemporary scientific worldview. Although many parapsychologists (e.g., Rao 1983) believe that only a major revolution in scientific thought could lead to the accommodation of psi, there have been attempts to reconcile such phenomena with modern science. For example, although it would seem that psi cannot occur without violating well-tested laws of physics – such as the law of conservation of matter and energy and the inverse square law of energy propagation (signal strength is proportional to the inverse square of the distance) – or violating the logical principle that an effect cannot precede its cause, *ad hoc* explanations of how psi might occur without such violation have been proposed (Collins & Pinch 1982). As an example, with regard to the presumed impossibility of seeing into the future, one could posit that what appears to be precognition is really psychokinesis: The individual uses PK to *bring about* the events he believes have been foreseen precognitively. In a similar fashion, one may be able to construct other *ad hoc* explanations to overcome all the various incompatibilities that appear to exist between physical science and parapsychology, although such contrived mechanisms are not likely to satisfy most scientists.

A more direct attempt to render psi compatible with contemporary science has been made through efforts to show that such phenomena are *not* inconsistent with quantum mechanics. In recent years, there has been considerable discussion in parapsychology, led by paraphysicists (parapsychologically oriented physicists) and philosophers, about some of the paradoxes of quantum mechanics and about how it is possible to suggest solutions to these paradoxes that imply the direct influence of the mind on matter, allowing for – or even *demanding* – psi (e.g., Oteri 1975; Schmidt 1975; Walker 1974; 1975).

This has generated negative reaction even within parapsychology (e.g., Braude 1979a), with some paraphysicists such as Phillips (1979; 1984) arguing that the orthodox view of quantum mechanics does *not* lead to paradoxes that necessitate the introduction of mental influences. Phillips describes the difficulty and the arbitrariness of interpreting the mathematical picture served up by quantum theory: "The predictions of quantum mechanics have been verified, and there is little doubt that the mathematical formalism is correct. Constructing a physical picture to correspond to the mathematics is much more difficult, and authors differ in what they find intuitively appealing and philosophically satisfactory" (1984, p. 298).

Even if quantum mechanics did allow for psi – a notion few mainstream scientists would be likely to accept at present – that would not in itself make the reality of psi any more likely. Flying cows are not inconsistent with quantum mechanical notions, but as far as we know, they do not exist. What is missing in such discussions of psi is the phenomenon itself. Until there is clear evidence that psi exists, it is surely premature to try to bend quantum mechanics to accommodate it.

3. If psi exists, how can it be detected?

There are three major sources of evidence for psi: (a) anecdotes of spontaneous personal experiences, (b) demonstrations by "gifted" psychics, and (c) laboratory experiments. The early studies of psi examined anecdotal reports in great detail, but gradually the realization grew that such evidence is just too unreliable to serve as data for science (Hövelmann & Krippner 1986; Rhine, L. E. 1977; Rush 1986a).

"Gifted" psychics have provided the most spectacular psi claims, both in the early days of psi research and more recently (Rush 1986b). For some parapsychologists (e.g., Beloff 1985), such demonstrations still stand as strong testimony to the reality of the paranormal. Yet, once again, this evidence is unsatisfactory in the extreme, because of both the history of fraud involving reputedly gifted psychics (e.g., see Girden 1978) and, more important, the fact that such psychics have as yet been unable to perform their feats under controlled conditions for neutral or skeptical investigators. For example, Uri Geller was taken by a number of parapsychologists (e.g., Beloff 1975; Cox 1976; Eisenbud 1976; Hasted 1976; Moss 1976; Puthoff & Targ 1974) to have genuine paranormal powers until a conjurer's investigations (Randi 1975) showed to most people's satisfaction that Geller was using trickery.

Some parapsychologists (e.g., Schmeidler 1984) insist that the fact that a psychic is caught cheating does not weaken the evidential value of those demonstrations during which the same psychic was *not* caught cheating. Given the rarity of such supposedly gifted individuals, it is not surprising that investigators are loath to terminate their research with an individual just because fraud has been detected on some occasions. However, it is no easy task to guard against fraud if a subject is determined to cheat, and what better indication is there of such determination than the subject's being caught at it?

It was because of dissatisfaction with both anecdotal evidence and uncontrolled demonstrations that Joseph Banks Rhine, in the 1930s, set up an experimental laboratory for the study of psi. The hope was that through rigorous application of the methodology of science, psi would soon be put on a solid empirical footing. Rather than simply relying on the ability of self-proclaimed psychics to demonstrate their skills, Rhine began the systematic study of both gifted and ordinary individuals in a number of "guessing" tasks in which probabilities of success could be calculated. If one makes a prediction, based on a probability model, as to how well a subject should score in a guessing task, or if one predicts the distribution of events whose occurrence depends on a random process (in Rhine's day, dice-throwing; nowadays, subatomic particle emission) which the subject attempts to influence mentally, then if all known normal forces have been ruled out, statistically significant departures from the prediction are taken to indicate the involvement of a psi process. Thus, experimental parapsychology, just as conventional psychology had done before it, took on a pronounced statistical flavour.

If one could reliably demonstrate departures from some statistical model, this would call out for explications. There would be no justification, however, for beginning with an explanation based on parapsychological concepts. If there were unobserved weaknesses in the controls, if some unknown process were involved (e.g., the use of some code based on silent counting, or the use of "silent" dog whistles that children, but not adults, can hear [Scott & Goldney 1960]), if there were equipment problems or biases in the random generator, if the statistical model were inappropriate, or if errors were made in the recording or analysis of the data, the paranormal explanation would be erroneous. Just as important, in the absence of a positive theory of psi, even if an observed effect is not due to artifact, one is left only with an anomaly. The availability of the psi hypothesis can distract the researcher from other, normal, explanations and thus impede the development of the understanding of anomalies (Blackmore 1983a).

What would constitute "solid" evidence of psi? Obviously, no evidence is ever 100% solid, because we can never be sure how new discoveries will change our understanding of processes that we currently think we understand. Furthermore, evidence that seems unconvincing or unimportant in the light of one theoretical worldview may be viewed as much more important if the prevailing theory changes.

An extraordinary degree of evidence is often demanded in support of extraordinary claims. We are generally less demanding of evidence in the case of claims that "fit" with existing theory or knowledge. When one is weighing evidence in law, the distinction is made between "beyond all reasonable doubt" and "on the balance of probabilities." The former, applied in criminal cases, demands virtual certainty of guilt; the latter, used in civil litigation, refers to the notion that the defendant is more likely than not to be guilty. Because psi is a concept that would probably revolutionize science (Rao 1983), most skeptics implicitly use the criterion of beyond all reasonable doubt, while accepting conclusions made on the balance of probabilities where only "normal" and non-controversial phenomena are involved. However, although the controversial nature of psi may justify the use of tougher criteria, this view has been attacked as being another tactic for denying legitimacy to controversial claims (McClenon 1984).

Before we accept that psi (even in the simplest sense as an anomaly) has been demonstrated in the laboratory, three important factors must be considered:

3.1. Internal validity. Psychologists use the term "internal validity" to refer to the degree to which experiments are free of the influence of extraneous variables that might introduce alternative explanations for the observed results (Berkowitz 1986). Most criticisms of experimental studies of psi concern internal validity: Randomization may be inadequate, sensory leakage (i.e., communication of information by normal sensory means) may have occurred, and so forth.

McClenon (1984) argues that such methodological criticisms of psi experiments are often unfair. By refusing to accept the shared assumptions that are implicit in any experiment, he says, the critic will sooner or later "ask for information that is no longer available, or for a degree of experimental control and exactitude that is desirable in principle but impossible in practice" (p. 89). Thus, the "perfect" ESP experiment is an impossibility, McClenon contends, for one can always suggest that the experimenter was incompetent or that trickery was involved (see also Honorton 1981). Despite McClenon's concerns, there is a considerable difference between making unsubstantiated charges of incompetence or trickery and pointing to methodological flaws. If the flaws are there, parapsychologists should run the experiments again – without the flaws – rather than argue about the motivation of the person who pointed them out.

Rather than rerunning the experiments correctly, a more usual response is to attack the critic. For example, critics have been chastised for pointing to flaws without demonstrating that these flaws are capable of generating the observed departures from chance (Honorton 1975; 1979; Palmer 1986a). This criticism does not stand up, for two reasons. First, critics are usually not advocating the acceptance of an alternate hypothesis but asking only that claims of psi be suspended until properly controlled studies are carried out (Akers 1984; Hyman 1981). Second, such flaws need not be the *cause* of the statistical deviations, but they *are* symptomatic of lax research standards (Hyman 1985b). One should hardly have confidence in the experimental controls if one is faced with evidence of violations of proper procedure. Akers (1984) uses the "dirty test-tube" analogy: A chemist would have little confidence in a colleague's findings if it were observed that a test tube used in the experiment was contaminated.

It is not so difficult to design and execute an experiment that is methodologically and statistically sound. Psychological experiments published in the better psychology journals stand in evidence of this.

3.2. Consistency. Before accepting the reality of a phenomenon, one generally looks for signs that there is a consistent pattern of results across experiments. The lack of any consistent pattern in the research findings is one of the most serious weaknesses in the evidence offered for psi (Blackmore 1983a). Unfortunately, it is standard practice in parapsychology to take one pattern of data as evidence for psi in one experiment, then to disregard its absence and take some other pattern as evidence for psi in another experiment.

3.3. Repeatability. Not only should there be consistency in the pattern of data across experiments, but individual experiments should be repeatable by others. Repeatability is an important safeguard, albeit only a partial one, against error or fraud (Sommer & Sommer 1984). Obviously, however, replication by itself is not enough. If someone is dishonest in the actual reporting of the research, reports of replication by the same author will not eliminate the dishonesty (Casrud 1984).

Yet, as Rao (1985) points out, repeatability is not a matter of primary concern in normal science. Only if some important and controversial finding is made is replication likely to be attempted, and this will often be undertaken by others who have competing theories that would not accommodate the finding. When observations are consistent with theory, replication is less important. However, as Murphy (1971) commented: "If the event is unclassifiable, then it is doubly important that it have a rational interpretation, that is, one that fits with the thought patterns of the contemporary human mind. If it has no clear rationality, its only chance of demanding scientific attention is replication" (p. 4).

On this basis, repeatability is, in general, less important in psychology than in parapsychology. Even so, psychologists pay far too little attention to the importance of repeatability (Epstein 1980; Fishman & Neigher 1982; Furchtgott 1984; Heskin 1984; Sommer & Sommer 1983; 1984); replication studies account for a very small percentage (3% or less) in leading psychology journals (Bozarth & Roberts 1972; Sterling 1959). This has led on occasion to the widespread dissemination of information that is subsequently found to be unreplicable (see, for example, Marshall & Zimbardo 1979; Maslach 1979; Schachter & Singer 1979).

Even when replication is attempted, its importance often depends on who conducts it. We are not likely to accept a wild claim supported by the research of only one person, whether that research has been replicated by that person or not (Hyman 1977a). Similarly, a failure to replicate by a student in a high-school science class will carry little or no weight, whereas a failure to replicate by a well-respected scientist will be much more seriously viewed (Collins 1976). It is also difficult to know just what constitutes a replication (Edge & Morris 1986); there are in fact several different kinds of replication that one can provide (Alcock 1981; Lykken 1968). Beloff (1984) differentiates between "weak" and "strong" replicability, where the former term refers to a situation in which an experiment or phenomenon has been independently confirmed by at least one other investigator, and the latter refers to a situation in which any competent researcher, following the prescribed procedure, can obtain the reported effect. Although parapsychologists have presented, as evidence for psi, studies that have been replicated by other parapsychologists, there has never been a psi demonstration that is replicable in the strong sense (Beloff 1973; 1984; Palmer 1985b). Indeed, parapsychologist/psychologist Susan Blackmore (1983a) recently referred to unrepeatability as parapsychology's *only* finding.

Of course, even if a psi experiment is replicated, that by itself does not mean the effect has a paranormal cause. On the other hand, the inability to repeat an experiment or a demonstration cannot by itself rule out the truth of the psi claim. Poor repeatability could conceivably stem from factors other than the nonexistence of psi (Palmer 1986b). It is possible that certain conditions are necessary for the production of psi, and given that no one knows just what these conditions are, it could be that an essential element is missing when an experiment fails to replicate. It has also been suggested that psi could turn out to be inherently unlawful (Palmer 1986b; Rao 1982), although this position is difficult to defend (Hövelmann & Krippner 1986). From this viewpoint, it has been argued that the quest for repeatability should be abandoned (Pratt 1974).

Despite the arguments about the relative unimportance of repeatability, the history of science demonstrates that unrepeatable experiments or demonstrations should be viewed with a very cautious eye. Most parapsychologists probably would not dispute this point. Indeed, the claim is made that the level of repeatability that has been demonstrated in parapsychology exceeds typical replicability rates in the social sciences; the strongest claim in this regard concerns the psi ganzfeld effect, for which replicability is said to be in the area of 50% (Honorton 1976; 1978b). This research is discussed in the next section.

In summary, then, although one cannot set precise standards that evidence of psi must meet, judgment should be suspended until there is at least some consistency among research findings from a body of methodologically irreproachable experiments, at least some of which are repeatable in Beloff's (1984) strong sense.

4. Is there any substantive evidence that psi exists?

Within parapsychology itself, there are arguments about the strength of the evidence adduced for psi. Some argue that no substantive evidence has yet been found (e.g., Parker 1978), whereas others consider the laboratory evidence for psi convincing (e.g., Schmeidler 1984); still others believe that psi can even now be harnessed – for example, to guide stock market investments (Targ & Harary 1984). On the whole, it would appear that most parapsychologists believe that psi has already been demonstrated. Schmeidler (1971) reported that almost 90% of her small sample of members of the Parapsychological Association indicated they believed that ESP had been established so firmly that any further research aimed only at demonstrating its existence would be uninteresting. Subsequently, in a survey of all 241 members and associates of the Parapsychological Association (which yielded a response rate of 84%), 68% indicated complete belief in the reality of psi (McConnell & Clark 1980). The average strength of belief across all respondents was 93%.

Many studies have been carried out and published that purport to provide statistical evidence for paranormal processes. However, even if we were willing to treat certain statistical deviations as evidence of psi, such evidence has been unsatisfactory: A number of recent analyses have demonstrated a serious problem with the quality of the methodology used in parapsychological research. For example, Akers (1984) cites a survey of 214 PK experiments (May et al. 1980), in which the authors concluded that none had been properly designed and reported.

In order to explore in more detail the state of the evidence in parapsychology, five major areas of contemporary parapsychological research will be discussed below.

4.1. Out-of-body experiences. Blackmore (1982; 1984), after carefully studying both the anecdotal and research literature on out-of-body experiences (experiences in which the individual believes that the physical body has been left behind and that travel through physical space is therefore unencumbered by limitations imposed by the flesh) and after conducting her own research, came to the conclusion that normal psychological theories are capable of accounting for such experiences and that nothing paranormal is likely to be going on.

4.2. Personality/attitudinal variables and psi. Akers (1984) evaluated 54 experiments that studied the influence of altered states and of personality/attitudinal variables on psi and that had been cited as significant confirmations of psi. He found that 85% of the experiments were seriously flawed, and even the 8 that were conducted with reasonable care were not methodologically ideal. The problems fell into several categories, including randomization failures, sensory leakage, inadequate safeguards against subject cheating, the possibility of errors in the recording of the data, errors in statistical analysis, and failures to report important procedural details. Akers concluded that these 54 experiments taken together were too weak to establish the existence of a paranormal phenomenon.

4.3. The psi ganzfeld effect. As mentioned earlier, studies of ESP in a ganzfeld (a condition of reduced sensory stimulation typically produced by covering a subject's eyes with halved Ping-Pong balls and shining a white light onto them while playing white noise into the subject's earphones) have been very promising in that they have appeared to demonstrate a replication rate of 50% or higher (Blackmore 1980; Honorton 1978b).

Hyman (1985b) has completed an exhaustive analysis of virtually all psi ganzfeld research, using a data base of 42 studies conducted between 1974 and 1981. Hyman's analysis leads him to conclude that the replication rate exhibited in this collection of studies is probably very close to what would be expected by chance. Several flaws of procedure – including less than adequate randomization, the possibility of sensory leakage, and erroneous statistical analysis – plagued these studies; not a single study was flawless, he reported. He suspects that most of these studies were not well planned, and he concludes that this data base is too weak to support any assertions about the existence of psi. However, Honorton (1985) disputes Hyman's conclusions, arguing that his assignment of flaws is itself seriously flawed, and he maintains that these studies do indeed indicate a significant psi ganzfeld effect.

Hyman and Honorton (1986) prepared a joint paper as a follow-up to the two papers discussed above. With reference to the data base discussed earlier, they agree that

> the experiments as a group departed from ideal standards on aspects such as multiple testing, randomization of targets, controlling for sensory leakage, application of statistical tests, and documentation. Although we probably still differ about the extent and seriousness of these departures, we agree that future psi ganzfeld experiments should be conducted in accordance with these ideals. (p. 353)

They go on to say that

> whereas we continue to differ over the degree to which the current ganzfeld data base contributes evidence for psi, we agree that the final verdict awaits the outcome of future psi ganzfeld experiments – ones conducted by a broader range of investigators and according to more stringent standards. (pp. 352–53)

Thus, although the ganzfeld studies have been offered as the strongest evidence for a repeatable psi effect, any conclusion about a psi ganzfeld effect must await future research carried out more carefully than these studies were.

4.4. Remote-viewing studies. In 1974 *Nature* carried an article by two physicists (Targ & Puthoff 1974) in which they described their successful demonstrations of "remote viewing," a talent by means of which subjects are able to describe geographical locations being visited by other people without having any normal form of communication with them. This putative skill is said to be within everyone's capability (Targ & Puthoff 1977). For a period of time, this research seemed to promise a breakthrough in the search for a demonstrable psi effect. However, Marks and Kammann (1978; 1980), unable to replicate the remote-viewing effect themselves, discovered serious flaws in the remote-viewing procedure – flaws that they argued accounted for the observed effects.

The principal flaw concerned the judging procedure: Judges were asked to match up a series of responses against a set of targets. Marks and Kammann argue that because the transcripts of the subjects' reports were not edited to remove cues that would assist the judges in identifying the targets, the judging procedure itself – and not any psi effect – produced above-chance matching of transcripts with targets. Tart et al. (1980) responded to this criticism by first having the transcripts edited to remove any possible extraneous cues, and then having them rejudged. They reported that this did not eliminate the remote-viewing effect. However, Marks and Scott (1986), after obtaining access to the relevant findings (they had until recently been denied access to the raw data), report that the editing of the transcripts had failed to eliminate all the extraneous cues and that enough cues remained to account for the above-chance scoring rate.

There have been other criticisms of the remote-viewing studies as well, including concerns about statistical problems that could give rise to above-chance scoring rates (Hyman 1977b), and about the lack of adequate controls and control groups (Caulkins 1980). A number of replications and extensions have been reported (e.g., Bisaha & Dunne 1979; Dunne & Bisaha 1979; Schlitz & Gruber 1981; Schlitz & Haight 1984); only the Schlitz and Haight (1984) study appears to avoid the weaknesses of the Targ–Puthoff series, but even here, there was no control condition to allow proper assessment of the background "coincidental" scoring rate.

Thus, the Targ–Puthoff series is too flawed to be of evidential value, and none of the subsequent published studies have been carefully enough controlled to bear testimony about psi.

4.5. Schmidt's random-event generator (REG) studies. For almost 20 years, Helmut Schmidt has been conducting research into the ability of subjects to predict or influence the radioactive emission of subatomic particles. His research enjoys generally high regard from other parapsychologists: Beloff (1980), for example, views some of Schmidt's research as being among the most evidential in all of parapsychology, despite his own inability to replicate Schmidt's findings.

Schmidt has published a considerable number of studies. Unfortunately, this investigator typically completes a study and then – rather than focusing on a given research question, or refining his measurements, or examining the effects of various parameters in that particular situation, or working with one type of generator over a period of time so that he and others can come to appreciate its idiosyncrasies – he moves on to a totally different situation altogether (Hansel 1980), changing the design and components of his generator as he goes along (Hyman 1981). This makes it very difficult for him or anyone reading his research reports to learn the limitations of his generator or his procedures.

Little of Schmidt's research is free from serious methodological shortcomings (Hansel 1980; 1981; Hyman 1981). Consider, for example, one of his initial studies (Schmidt 1969b), which has been favorably cited many times in the parapsychological literature. The situation was as follows: A subject was seated before a panel of four lights and four corresponding buttons. On each trial, the subject would press one of the buttons to predict which light would next illuminate, something that would be determined by particle emission from a strontium-90 source. The light would then illuminate, giving immediate feedback. If the light corresponded to the depressed button, it was a "hit."

In the first experiment in this report, Schmidt combined the results from his three subjects and obtained a hit rate significantly higher than would be expected by chance: 0.261 as compared to 0.250 ($p < 2 \times 10^{-9}$). In the second experiment, subjects were allowed to choose to try to make a high or a low number of hits. Here, the combined scoring rate of three subjects was 27%, again significantly higher than chance expectation ($p < 10^{-10}$).

Both experiments suffered from less than optimal experimental control; as in most of Schmidt's studies, subjects were usually unsupervised, and there was a general lack of rigour in the control of experimental conditions. Hansel (1980) objected to the fact that the exact numbers and types of trials undertaken by each subject were not specified in advance, and also to the fact that the equipment, although partially automated, did not rule out cheating during data classification.

There is a more fundamental concern about these experiments: the target series (Hyman 1981). Schmidt compared the subjects' hit rates to chance expectation, but this assumed that the target series was random. (Particle emission is presumably random; the output of his generator was not necessarily so.) Schmidt's randomization checks were carried out on target strings much longer than those used in the experiments, and therefore did not allow the detection of possible short-term biases in the generator which could give rise to nonrandom target strings. Because immediate feedback was provided throughout the experiment, and because subjects were free to "play" with the equipment and to decide when to start and stop a given session, any undetected short-term bias in the generator might give the subject the impression of being "hot" and therefore lead him to initiate a session, which he would probably end once he seemed to turn "cold." This, of course, could produce above-chance scoring rates.

It would therefore be important and appropriate to analyze the *actual* target sequence in terms of how well it conformed to what would be expected by chance. However, were one to find that the target sequence was nonrandom, this could, after the fact, be taken as evidence of PK. Indeed, Schmidt reported that after the testing one subject said he had tried to affect the outcome rather than just predict it; he had tried to produce more illuminations of lamp no. 4, he said. It was found for this subject that there was indeed an excess of 4s in his target series. No indication is given in the report as to whether this analysis of targets was carried out for other subjects, and if not, why not. However, Schmidt subsequently used this same piece of apparatus in a PK experiment (Schmidt & Pantas 1972) in which the only task was to try to influence the machine to produce an excess of 4s! Above-chance scoring rates were reported in that instance as well, which led Schmidt again to conclude that psi was operating. The skeptic is left wondering whether that apparatus simply produces an excess of 4s from time to time. Certainly, nothing can be concluded from such reports until more is known about the target series produced by the generator.

Thus, a study that seems at first to offer considerable evidence of an anomalous process is found to be badly flawed. It would make sense for Schmidt to redo the study, taking steps to make these criticisms unnecessary. Generally lacking in Schmidt's studies is a proper control condition: One should generate *pairs* of runs, with one run designated, on the basis of some random procedure such as the toss of a coin, as the experimental and the other as the control for each trial (Hansel 1981).

The problems in this study recur over and over in Schmidt's research (Hansel 1980; 1981; Hyman 1981). Only one of his studies appears well designed (Schmidt et al. 1986). However, we must wait to see whether the psi effect apparently obtained in this very recent study stands up to replication. There have been many psi studies (e.g., Targ & Puthoff 1974) in the past that at first appeared beyond reproach, only to be found later to be seriously flawed.

In summary, these various areas of research are plagued by methodological and statistical flaws of one sort or another. Until research is undertaken that is methodologically well planned and well executed – as Hyman and Honorton (1986) recommend with regard to the ganzfeld – there is little point in debating whether or not the existing evidence establishes a case for psi.

5. Does parapsychology follow the rules of science?

Of course, by using the term "rules of science," one could open up all manner of dispute because of the difficulty that exists in listing those rules or in demarcating science from pseudoscience (e.g., see Bunge 1984; Edge & Morris 1986). Rather than tackle that conundrum, it is more profitable to examine several aspects of parapsychological endeavor that appear to run counter to the spirit of scientific inquiry; each is discussed below:

5.1. Unfalsifiability. There are a number of principles in parapsychology that can be used to explain away failures to find empirical support for a hypothesis, thus creating a situation of unfalsifiability:

1. Perhaps the subject did significantly worse than expected by chance. If so, this may be taken as evidence of psi, because it seems to be *psi-missing*, something which occurs so often that it is now taken to be a manifestation of psi (e.g., Crandall & Hite 1983).

2. If outstanding subjects subsequently lose their psi ability, or if subjects do more poorly toward the end of a session or of a series of trials, this is labeled the *decline effect* (e.g., see Beloff 1982). Rather than being taken as a possible consequence of either statistical regression or the tightening up of controls (when that has occurred), the decline effect often takes on the power of an explanation, because it has come to be viewed as a property of psi. For example, the decline effect in one experiment was interpreted as a "sign of psi" that was taken to strengthen the claim of a genuine psi effect (Bierman & Weiner 1980).

3. In a related vein, Schmeidler (1984) reports that PK effects are often strongest just *after* a session has terminated or during a subject's rest period. Rather than ignoring data accumulated after the session is over, this is taken to reflect another psi phenomenon, and has been given two names – the "linger effect" and the "release of effort effect." If this is to be taken seriously, then all researchers should report not only the presence of such an effect, but its absence as well: were this done, the frequency of the effect may well turn out to be within the bounds of normal statistical expectation.

4. Some parapsychologists seem consistently to obtain the results they desire whereas others are unable to find significant departures from chance (Palmer 1985b). The failure of one researcher to obtain significant results using the same procedure that yielded significant results for another researcher, rather than being taken as a failure to replicate or as a hint that extraneous variables may be producing artifactual results, is often interpreted in terms of the *experimenter effect*. This effect is so common in psi research that it has even been described by one parapsychologist as parapsychology's one and only finding (Parker 1978)! To *describe* the fact that two researchers obtained different results by calling it an experimenter effect is quite appropriate. After all, the experimenter effect as such is by no means unique to parapsychology, and a great deal has been written on the subject with regard to research in psychology and other domains (see Rosenthal & Rubin 1978). However, in psi research the term is all too often used more as an *explanation* than as a description, and that is because it is considered that the effect may result not only from experimenter error (in that one experimenter may be more successful in obtaining psi effects than another because he unwittingly allows more artifacts to contaminate his procedure), or from differences in personalities (in that some experimenters may put their subjects into a more comfortable and psi-conductive frame of mind than others), but also from the psi influence of the experimenter himself (Krippner 1978a; Palmer 1985b; 1986b). If psi exists, of course, it would only make sense that the experimenter, who naturally wants his experiment to succeed, might unknowingly bring his psi influence to bear, whereas a skeptical or neutral experimenter might not use psi at all, or might use it to prevent the appearance of a subject psi effect. This whole problem leads Palmer (1985b) to describe the experimenter effect as the most important challenge facing parapsychology today. It is hard to imagine scientific inquiry of any sort if the results of the investigation are determined by the psychic influence of the investigator (Alcock 1985; see also Krippner 1978b).

The experimenter effect (or the experimenter psi version of it) provides a powerful method for undermining failures to replicate, and is sometimes resorted to for just that purpose. For example, when Blackmore (1985), a devoted parapsychologist for many years, found herself becoming increasingly skeptical about psi as a consequence of her inability to produce experimental evidence for it, she noted that "many parapsychologists suggested that the reason I didn't get results was quite simple – *me*. Perhaps I did not sufficiently believe in the possibility of psi" (p. 428).

In summary, it is the way such "effects" are used – and not, in principle, the research procedures – that vitiates the scientific respectability of parapsychology, for they make the psi hypothesis unfalsifiable by providing ways to explain away null results and nonreplications. These descriptive terms have mistakenly come to be taken as properties of psi, which leads to the circularity of explaining an observation by means of the label given to it. Moreover, as important properties of psi, their *nonappearance* in a psi experiment should weigh against any conclusion that psi has occurred; this never happens in the parapsychological literature.

5.2. All things are possible. Another aspect of parapsychology that makes critics uncomfortable is what seems to be almost an "anything goes" attitude, with no speculation seeming too wild. For example, so-called observational theory based on paraphysical interpretations of quantum mechanics, predicts that random events can be affected simply by being observed, even if the observation occurs at some time in the future (see Bierman & Weiner 1980). In line with this notion, studies have been done which claim to show that subjects can exercise an influence backwards in time ("retroactive PK") so as to affect the choice of stimulus materials preselected for the study in which they are participating (e.g., Schmidt 1976). This also means, of course, that the present is possibly being influenced by future events (Martin 1983). A "checker effect" has also been postulated, in which ESP scores may be retroactively and psychokinetically influenced by the individual who checks or analyzes the data (Palmer 1978; Weiner & Zingrone 1986). Schmidt (1970c) reported that cockroaches were able to influence a random-event generator in such a way as to cause them to be shocked *more* often than would be expected by chance. He suggested that perhaps his own psi, fueled by his dislike of cockroaches, accounted for the increase, rather than a decrease, in shocks.

Not only can psi apparently transcend temporal boundaries, it also seems that no effort, no training, and no particular knowledge are required to use it. Indeed, modern PK studies appear to indicate that psi is an *unconscious* process, but a goal-oriented one in that it helps the individual attain desired objectives: Success in a PK experiment does not require knowing anything about the target, or even knowing that one is in a PK study (Stanford 1977). Thus, psi appears to operate very much like wishful thinking. For example, going back to the Schmidt (1969b) study, all that was needed, it seems, was for that one subject to *wish* for a particular light to come on and it would light up statistically more frequently than the others. (Of course, when subjects do score above chance, neither they nor anyone else can say which hits were brought about by psi and which were the consequence of chance.)

As I have argued earlier (Alcock 1984), the fact that no physical variable has ever been shown to influence the scoring rate in psi experiments (Rush 1986c), combined with the apparent total lack of constraints on the conditions under which psi can be manifested (whether forward in time, backward in time, across thousands of miles, between humans and objects, between humans and animals, or even between animals and objects), serves to weaken the a priori likelihood that psi, as any sort of force or ability, exists. After all, most psi experiments are very similar, in that all that is typically done is to examine two sets of numbers, representing targets and responses in an ESP experiment or outcomes and aims in a PK experiment, for evidence of a nonchance association. It may simply be that the enterprise of parapsychology generates, from time to time, significant statistical deviations – be they the result of artifact, selective reporting, or whatever – which are then independent of the research hypothesis, so that no matter what the researcher is examining – the effects of healing on fungus, PK with cockroaches, ESP across a continent, or retroactive psi effects – the likelihood of obtaining significant deviations remains the same. (For example, if an REG produces an excess of 4s on a short-term basis, and if the procedure allows subjects to tap into this, then it should make no difference in principle whether the targets are generated on-line or were recorded a week earlier: If the subject aims for more 4s, he will obtain them.) Difficulty in replication by other researchers using their own equipment or slightly different procedures would, of course, follow from such a state of affairs, as would the experimenter effect.

This psi-as-artifact notion is not offered as an empirically testable hypothesis. I only mean to show that the lack of constraints on the appearance of psi undermines rather than strengthens its credibility. It would be hard enough to accept that a philosopher's stone can turn base metals into gold, as alchemists believed. It would be harder still to believe that it can turn *anything* into gold and that anyone can use it without any training.

5.3. Lack of rapport with other areas of science. Parapsychology, despite its efforts to find common areas of interest with other research fields (see the *Handbook of Parapsychology* [Wolman 1977a]), has failed to establish any genuine overlap with other disciplines, because, so far at least, other disciplines do not seem to *need* psi. If "normal" explanations for strange physical or psychological phenomena were exhausted, and/or if the influence of the researcher's consciousness appeared to have an effect on the way matter behaved in "normal" experiments, then a much greater number of scientists might be more open to the possibility of psi. Indeed, if parapsychologists are right about psi, then the well-tested theories of physicists and neurologists are wrong (Hebb 1978). It is perhaps noteworthy that the claims that psi can influence radioactive decay do not come from particle physicists in the course of their everyday work.

6. Are the critics fair?

Some parapsychological proponents, such as Child (1985), argue that few in "normal" science bother to immerse themselves in the details of parapsychology, and instead gain a false or misleading impression from the accounts given by their colleagues who serve as critics of the field. Such critics are accused of unfair tactics, such as (a) arguing that unless fraud can be ruled out, it is the most parsimonious explanation of psi claims; (b) setting higher standards for parapsychological research than for research in the realm of normal science; and (c) simply rejecting the possibility of psi out-of-hand (see Collins & Pinch 1979).

Charles Tart (1982; 1984), a former president of the Parapsychological Association, suggests that there is an emotional basis for critics' unwillingness to welcome parapsychology into the scientific fold, an argument that has been repeated by Schmeidler (1985) and Irwin (1985b), among others. Tart posits that a widespread and unconscious *fear* of psi has developed either because strong psi ability would disrupt social functioning (because we would have access to one another's true feelings and thoughts) or because of what he calls "primal conflict repression": A mother often feels angry toward her child but keeps her cool and speaks to the child in a positive, supportive way. The child, if psi is already operating, is faced with a frightening conflict of messages and learns to repress psi altogether so as to avoid the information channel creating such conflict. Targ and Harary (1984), on the other hand, argue that skeptics base their opposition not on rationality but on religious conviction.

Suggestions about fear and religious conviction are too weak and *ad hoc* to require rebuttal. Collins & Pinch's (1979) concerns, on the other hand, are important. However, they could be equally relevant to any controversial claim, and thus nothing abnormal seems to be going on in the critical reactions to parapsychology. The scientific arena is a tough one; many ideas march in to do battle; some survive, but just as many perish. Numerous other controversial claims have faced hostility and even derision from scientists; some of these have won out (e.g., continental drift – see Hallam 1975); others (e.g., polywater – see Franks 1981) have not. Psychologists were at first unwilling to believe in the notion of biological preparedness with regard to learning (i.e., the idea that organisms, including humans, are biologically prepared to learn certain kinds of aversions more rapidly than others), and the leading journals refused to publish research reports on the subject, reports that are now viewed as being among the most important in their field (Seligman & Hager 1972). This concept is now part of mainstream psychology. Many psychologists also refused to believe in biological constraints on intelligence (or, at least, racially determined ones); and as a result of such dogged refusal to believe, the fundamental studies in this area – reported by Sir Cyril Burt – were eventually exposed as fraudulent (Kamin 1974). When, in the late 1960s, Neal Miller announced that he and L. Dicara had demonstrated operant conditioning of heart rate in rats (Miller & Dicara 1967), many experimental psychologists refused to believe it, despite Miller's high reputation as an experimental psychologist. Ultimately, Miller himself, when subsequently unable to replicate his own studies, publicly withdrew his claims (e.g., Miller 1978). *Science* refused to publish, on the grounds that it was erroneous, the initial research of Solomon Berson and Rosalyn Yalow (Yalow subsequently won the Nobel Prize) on the insulin-binding antibody, research that was fundamental to the development of the radioimmunoassay technique (Garfield 1986; Yalow 1978). Albert Einstein absolutely refused to believe that "God plays dice," despite the implications of quantum mechanics; he chose to believe the theory to be in error due to incompleteness. Science is full of such examples.

This process, although sometimes seemingly cruel and dogmatic, is perhaps necessary to allow scientists to focus on claims that appear most promising, rather than being distracted by others that appear to have little to recommend them. Sooner or later in science, it seems, the truth will out, and error falls by the wayside. Even acupuncture, long regarded as being nothing short of superstition, is now regarded as capable of producing limited pain relief (Zusne & Jones 1982).

If the insulin-binding antibody, biological preparedness, and acupuncture analgesia won accommodation in science, it is because the evidence for them became so strong that they *had* to be accommodated. A century of parapsychological research has gone by, and the evidence for psi is no more convincing now than it was a century ago.

It seems accordingly that parapsychologists who attack scientists and critics for their refusal to recognize the importance of psi and of psi research are attacking the messengers because they cannot accept the messages they bear. Suppose that instead of psi, parapsychologists were promoting a cure for baldness, but that the amount of hair produced by the treatment was tiny and detectable only by some researchers, sometimes. If the effect is unreliable and unrepeatable, if it also contradicts all that is known about hair growth and alopecia, and if there is no theoretical mechanism put forth for the putative effect, then one would hardly expect the scientific community to cheer the end of baldness. Science will never take parapsychologists simply at their word; they must offer a clear, replicable demonstration of a basic phenomenon in order to gain acceptance in science.

Moreover, one can seriously challenge the claim that practitioners of normal science do not give, or have not given, parapsychology its day in court. As was mentioned at the outset, a number of professional scientific organizations have invited parapsychologists to address them or have set up symposia on the subject. True, parapsychological ideas have hardly been embraced with open arms, but that does not mean that scientists are motivated by fear or blind prejudice or ignorance or distorted interpretations purveyed by unreliable skeptics.

Indeed, when parapsychology began to take shape as a serious research field, a good number of psychologists and others immediately took up the challenge of investigating claims of spiritualistic communication, telepathy, clairvoyance, and so on. All that was lacking to make parapsychology part of mainstream psychology was evidence that there was a phenomenon to investigate. At the Fourth International Congress of Psychology, held in Paris in 1900, an entire section was devoted to psychical research and spiritualism, and the president, Ribot, announced the founding of a psychical research institute in Paris (L'Institut Général Psychique) (McGuire 1984). Membership in this institute included a number of prominent psychologists such as Janet, Richet, James, and Tarde. In 1895 Binet published some case studies of telepathy. However, as McGuire (1984) points out, psychologists were already becoming very uneasy about the growing link between psychical research and spiritualism; this mistrust began to show itself at the Fourth Congress, and subsequently many French psychologists began to turn their backs on psychical research.

Psychologists Pieron, Janet, and Dumas conducted a number of seances in which they reexamined mediums who had produced positive outcomes in earlier studies at the Institut Métapsychique. One medium was caught flagrantly cheating, and these psychologists concluded that no psychical phenomena had been observed under the carefully controlled conditions. LeBon offered a large reward to anyone who could produce the mediumistic effects in his laboratory, but once informed of the stringent controls, no one ever underwent the test (McGuire 1984).

The American Society for Psychical Research was set up in 1885 to examine apparent psychical phenomena (Moore 1977). Its officers included prominent psychologists such as Prince, Hall, Jastrow (later to become an outspoken critic), and James. When they failed to find any evidential basis for mediumistic claims, most members lost interest; the group was disbanded, and its remnants merged with the British Society for Psychical Research (SPR). James continued to support and believe in psychical research, and later became president of the SPR.

In the 1930s, parapsychology had another opportunity to persuade mainstream science about the importance of psi research. A poll conducted in 1938 found that 89% of psychologists felt the study of ESP was a legitimate scientific enterprise and 79% felt such research was a proper subject for psychologists (Moore 1977). In that same year, a round-table discussion of parapsychology was sponsored by the American Psychological Association. Parapsychologists did not succeed in their attempts to gain the psychologists' support for the study of psi.

The 1970s provided another period when mainstream science seemed ready to give parapsychology a chance. As mentioned at the beginning of this article, the Parapsychology Association had gained affiliation with the American Association for the Advancement of Science in 1969. In 1974 one of the world's leading scientific journals, *Nature*, published an article by paraphysicists Targ and Puthoff in which they detailed their claims about scientific evidence for the paranormal, based largely on research with Uri Geller (Targ & Puthoff 1974); true, the journal did precede the article with an editorial disclaimer, but the research nonetheless appeared. Although some parapsychologists were irked by the editorial "inoculation" *Nature* provided for its readers, such a disclaimer proved to have been prudent, because, as discussed earlier, Uri Geller was subsequently exposed as a fraud (e.g., Randi 1975).

Although mainstream psychological journals continue to be reluctant to publish parapsychological research, that is not to say that these journals are totally closed to parapsychologists; occasionally articles do appear (e.g., Layton & Turnbull 1975). *American Psychologist* recently published an article (Child 1985) that presented, along with his criticisms of skeptics' interpretations of parapsychological research, the results of a meta-analysis of the classic Maimonides dream studies. Child concluded that something important is going on, although, in my view, his analysis is unlikely to impress many psychologists. Parapsychology was discussed in an open-minded fashion, albeit very briefly, in a recent issue of the *Annual Review of Psychology* (Tyler 1981). Since 1950, more than 1,500 parapsychological papers have been abstracted in *Psychological Abstracts*, which is published by the American Psychological Association (McConnell 1983).

What more should parapsychologists expect, given the track record they have produced? I am of the strong opinion that rejection of, or dissatisfaction with, paranormal claims is not based on narrow, dogmatic prejudice, but on the fact that after a century of research, there is still nothing substantive to show!

7. Is rapprochement between psychology and parapsychology possible?

In 1982 psychologists Zusne and Jones's *Anomalistic Psychology* was published. This book constituted a milestone in the course of interaction between psychology and parapsychology by virtue of its attempts to establish a framework for the psychological study of the phenomena taken by parapsychologists to be paranormal. Blackmore (1983a), coming from the parapsychological side, and just as she was renouncing her belief in the psi hypothesis, also called for the study – within psychology – of the experiences that appear to people to be paranormal. Palmer (1986a) calls for a collective focus by skeptics and parapsychologists on finding explanations for anomalous experiences and phenomena, whether the explanations prove to be mundane or not. These actions may reflect what Truzzi (1985) views as a movement toward rapprochement between psychology and parapsychology.

Unfortunately, I doubt that such a rapprochement will ever occur, for I believe that those in parapsychology who move closer to the skeptical side will fail to draw the rest of parapsychology along with them. That is not to say that there will not be cooperation between psychologists and parapsychologists in the study of anomalistic experiences, something which should be strongly encouraged; nor is it to deny whatever movement there has been toward better mutual understanding and respect.

However, finding explanations for ostensible anomalies is not what parapsychology is really about for most parapsychologists. If it were, much more effort would be made to try to find psychological and neuropsychological explanations for such experiences before even contemplating the radical psi hypothesis. (Indeed, one must wonder why parapsychologists seem not to concern themselves with the actual *experience*, or with *how* such experiences are generated, or with *how* the supposed phenomena work [Scott 1985]. Why, for example, do they not set out to try to produce in subjects the subjective impression of telepathy, instead of merely concluding that subjects in a guessing task must have experienced telepathy on some of the trials? Studying guess rates is *not* the study of the telepathic *experience*.)

If parapsychology is not primarily motivated to explore anomalies in an open-minded fashion, what *is* its motivation? Why does parapsychology persist after a century of failing to produce compelling evidence of psi? Why does the psi hypothesis survive? To be fair, of course, normal science does not reject working hypotheses just because they fail to be confirmed empirically – although they rarely, if ever, show such longevity. For example, Harvey's theory of the circulation of the blood depended upon the existence of capillaries, and such capillaries could not be observed with the naked eye; but the failure to observe them did not lead to rejection of the theory. Investigators continued to seek them until, with the aid of microscopes, they were at last discovered (Gregory 1981). Yet, there is a difference between, on the one hand, not giving up a preferred hypothesis when that hypothesis seems to promise more explanatory power than existing theories about a range of observations and, on the other hand, the discounting of failure to find expected statistical deviations in a psi experiment. In the latter case, one is trying to establish the existence of a phenomenon that is *not* required by the existing body of scientific data, nor is it predicted by theory, nor would it simplify or clear up current anomalies in physics or psychology or biology.

The dispute about psi reflects the clash of two fundamentally different views of reality. The first of these is the materialistic, monistic view that the human mind is some sort of emergent manifestation of brain processes, whereas the second is the dualistic position that maintains that the human mind/personality is something beyond the stuff of atoms and molecules. Parapsychology grew out of the second of these; it developed directly from attempts, both in Europe and the United States, to put the postmortem survival of the human personality on a sound scientific footing (Cerullo 1982; Mauskopf & McVaugh 1980; Moore 1977). It is the search for the soul – not the Soul as it is described by various religions, and perhaps not even the secularized soul sought by the psychical researchers of the late nineteenth century during the heyday of spiritualism (Cerullo 1982), but a soul all the same. Because, if the mind can operate separately from the physical brain, as the psi hypothesis would suggest, then it possesses much of what has been ascribed to the soul.

Most religions teach that the Soul survives death in some form. The question of survival of the parapsychologists' "soul" or "mind" or "personality" after death is, even many leading parapsychologists agree, an important question for parapsychology to consider (e.g., Krippner 1983; Palmer 1983; Roll 1982). Blackmore (1983b) suspects that just as it was the *fundamental* question to many of the early psychical researchers, it is still so for many of her fellow researchers today.

Thus, it is important in any debate about parapsychology to make clear just what is being debated. Is the debate about whether or not there exist "natural" phenomena that science has so far failed to recognize, or is the debate about whether or not dualism, as opposed to materialistic monism, is the correct view of nature and of mankind's place in nature? Or, is the first question very often the surface issue, while the hidden agenda is the question of dualism?

8. Conclusion

"Either parapsychology is a harvest of false illusion, or the meat and fibre of biology, the focus of psychology, and even the material conception of physics on which all science stands" (Walker 1984, p. 9). These words by a paraphysicist should remind us that the existence of psi is no trivial matter. Yet, to accept the reality of psi, we must accept that some force or process exists which cannot at this time be described in terms of positive properties, but only in terms of what it is not; a force which is capable of allowing for direct communication between two brains, regardless of the distance between them; and which allows the mind directly and often unconsciously to influence matter in such a way as to gain some desired goal, again without any effect of distance, physical barriers, or even time. To accept the reality of psi, we must discount a hundred years of failure to find substantive evidence; there is not a single demonstration that is repeatable in Beloff's "strong sense." We must also accept that there are fundamental problems with well-tested physical and neurophysiological theories. We must accept all this in the face of the inability of parapsychologists to sort out whether, in a given experiment, a statistical deviation is due to PK or to ESP, whether it is due to the subject or to the experimenter, and whether the source of psi is acting in the past, the present, or the future. Furthermore, we must overlook the fact that even the best research programs in parapsychology are seriously beset by methodological weaknesses. We must ignore history as well, for as Hyman (1981) points out, each generation of parapsychologists has put forth its current candidates as providers of proof of psi – experiments that supposedly should have convinced any rational person were he to examine the evidence fairly. Yet, these candidates keep changing, and if prior history is a reliable guide, today's most promising research programs in parapsychology may well be passé in a generation or two.

If parapsychologists really are dedicated to the study of anomalous experience, then it should make more sense to follow Blackmore's (1983a) lead and focus on the anomalies while putting the concept of psi aside until, if ever, it is needed. This is unlikely to happen, however. Psi has been postulated not because normal psychology is incapable of accounting for people's apparently psychic experiences, nor because of inexplicable findings in physics or chemistry; nor is it the logical outgrowth of some compelling scientific theory. Rather, the search for psi is now, as it has been since the formal beginning of empirical parapsychology over a century ago, the quest to establish the reality of a nonmaterial aspect of human existence – some form of secularized soul.

All that is needed to turn the attitude of the scientific establishment from doubt to serious interest with regard to psi is to produce some clear, substantive evidence of a psychic phenomenon. Without it, parapsychology can never become a science.

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The evolution of science and "principles of impossibility"

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More than 2,000 years ago, the ancient Greek thinker Aristotle said, "Nihil est in intellectu quod non primus in sensu" (nothing can be understood by the intellect which is not first perceived by the senses). Due to this affirmation, he became, contrary to his own expectations, the international and eternal leader of all critics of ESP.

The Aristotelian dictum is, openly or not, the logical basis of all criticism of ESP. For example, the eminent nineteenth-century physiologist Von Helmholtz announced: "Neither the testimony of all the Fellows of the Royal Society, nor even the evidence of my own senses would lead me to believe in the transmission of thought from one person to another independently of recognized channels of sense."

There are "principles of impossibility," that is, systems of taboo in science. The Nobel Laureate physicist Sir George Thomson enumerated at least seven fundamental "principles of impossibility" (Thomson 1955), including the law of conservation of matter and energy, and the law of increasing entropy of closed systems, that is, the principle of chaos affirming the tendency of order to disappear. He remarked, however, that the most advanced and complicated discoveries take place in science when restrictions such as the "principles of impossibility" are ignored.

The "principles of impossibility" must have the potential to be refuted; otherwise they will transform into a system of dogmas like a religion. Certainly, scientific dogmas represent a brake on the progress of science.

Nevertheless, to study the complicated and changeable world in which we live, it is necessary to stop the movement and to separate explored from nonexplored phenomena. This is exactly the first step in mechanistic and formal thinking. "Principles of impossibility" are needed for the development of science, but they may obstruct its evolution. Undoubtedly, neither Aristotle nor Von Helmholtz, unlike present-day critics of ESP, had information about penetrating radiations and physical fields. At present, it is safe to say that these interact not only with inorganic substance but also with living matter, including the brain. Whereas human knowledge is formulated in terms of sensory perception, a tremendous invisible and inaudible world likewise surrounds us. The evolution of science is directly connected with the invention of instruments that translate information from an extrasensory "language" (X-rays; infrared, ultraviolet, and radio waves; ultrasound, etc.) into a "language" of sensory perception.

As to the brain, our current knowledge of how it works is less complete than our knowledge of the satellites of planet Neptune. Can the brain "translate" ESP information into a "language" of sensory perception directly? Which physical fields or penetrating radiations can transmit ESP? Can we adequately approach the understanding of ESP on the level of contemporary scientific models? All we can say about Aristotle's dictum is that it is not the final truth today. There are special devices for the blind which imitate "perception" by directing simple forms (e.g., lines, circles) via shortwave electromagnetic radiation directly onto the visual cortex of the brain.

Apart from Aristotle's dictum, there is at least one more principle hindering the development of parapsychology: the affirmation of the French medieval thinker René Descartes, "Cogito, ergo sum" (I think, therefore I am). The principle of rationalism denies the existence of the unconscious. But the unconscious, as well as the conscious, is involved in human experience, and it is the unconscious that seems to connect with ESP (Freud 1922; Ullman 1976; Ullman & Krippner 1970). In describing the psyche, the eminent physicist Niels Bohr suggested considering the conscious and unconscious mind in a manner much like the wave–matter dualism in physics (Bohr 1955).

Bohr's suggestion is a good example of dialectic or unmechanistic thinking. In my opinion, both those who are most critical or psi and the zealous believers in psi are in some way displaying mechanistic thinking as opposed to realistic thinking. The target articles by Alcock and by Rao & Palmer illustrate this. Despite its tendency toward the mechanistic view, Alcock's article is interesting enough and contains much useful information. I would particularly like to add to the list of very prominent scientists who have supported parapsychological research the name of the great German philosopher George Wilhelm Hegel. He analyzed such psi phenomena as healing, dowsing, and clairvoyance, using dialectic analysis, and included psi in his general approach toward the evolution of the psyche (Hegel 1817/1956).

Regarding a reasonable definition of paranormal phenomena, one can say that scientists have not succeeded in developing reasonable definitions of other natural phenomena. For example, it is known that even the definition of electricity is still problematic. Experts usually answer the question "What is electricity?" with "It is the directed flow of electrons," but when asked "What are electrons?" they answer, "Electrons are the smallest particles of electricity." In developing a definition of "paranormal" or "anomalous," perhaps we should ask first of all, "What is normal?" This is difficult, of course, because our conception of "normal" changes constantly.

The changes in our concept of normality seem to be a consequence of the development of the psyche. This development is displayed not only in our view of "normal." More important, the psyche changes our environment, that is, the material aspects of human existence. For at least one million years, the human mind has evolved while the human body has remained unchanged. How can the nonmaterial mind rebuild the material environment? Does the nonmaterial aspect of human existence really exist? Is psychokinesis only a piece of material law that operates on the level of individual human beings? The science of the future must provide answers to these questions.

In my opinion, the target article of Rao & Palmer reflects more adequately the real situation in contemporary parapsychology. Clearly, the success of one branch of science depends on its integration with the other branches of science.

Parapsychology is science, but its findings are inconclusive

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According to Rao & Palmer (R & P), psi anomalies may either represent (1) "hidden" artifacts or (2) a new principle of nature. R & P clearly favor (2). I suspect, along with Alcock, that psi effects arise from (1) and that the "hidden artifacts" are the ordinary, garden-variety sorts of artifact that can also be found in other areas of social science (e.g., lack of random assignment, uncontrolled variables, errors in data analysis, reporting errors, fraud).

Are R & P even claiming that there is evidence for the paranormal? Initially, they seem only to be claiming evidence for a communications anomaly. The anomaly might have a paranormal explanation, but it might also have a normal explanation; hence, they are taking an "open approach," and considering a variety of hypotheses. The authors claim that this "open approach" characterizes the parapsychological community as a whole.

Later, however, R & P imply that the data require some sort of paranormal explanation. This is apparent from their spirited defense of the Schmidt (1969a; 1969b) experiments, which they see as "probabilistically conclusive," and from their assertion that the experiments of Schmidt and other parapsychologists are statistically replicable. R & P do acknowledge that critics could "have reasonable grounds" for refusing to accept paranormal explanations. However, this would apparently be possible only if the critics had "a great deal of a priori skepticism."

The ambiguities in R & P's target article probably reflect the differing views of the two authors. Palmer's research does reflect the "open approach." He has, for example, actively explored the role of sensory cues in ganzfeld ESP effects (Palmer 1986d). Rao has been somewhat less open to normal explanations. He is inclined to argue that the evidence for psi is "inescapable" and that criticisms of the field are "unfair" and "false" (Rao 1978a, p. 270). Like the majority of parapsychologists (see McConnell & Clark 1980), Rao is firmly convinced that normal explanations will not suffice.

As evidence of the "ostensibly paranormal," R & P cite data from four areas of research: Schmidt's successful research with REGs (random-event generators), other research with REGs, ganzfeld studies, and differential effect studies. But how clean are these data bases? We do not know about the "other REG" and "differential effect" data because, according to R & P, "flaw analyses have yet to be reported." My own reading, however, tells me that many of the studies in question are far below the quality of Schmidt's work.

Until the REG and differential effect studies are evaluated, R & P's case will rest entirely on (1) Schmidt's studies and (2) the ganzfeld studies. These are legitimate contenders, because in both areas there has been some evaluation of quality.

Schmidt's research. R & P argue that Schmidt's (1969a; 1969b) results are "probabilistically conclusive." I must disagree. The research of a single scientist, no matter how high the quality of the work or how good the reputation of the investigator, cannot be conclusive of anything. The validity of any research depends on the extent to which the findings can be independently replicated.

R & P do claim that REG results are statistically replicable, at a rate of 21%. In quoting this figure, however, the authors are not implying that any given experimenter can succeed in one out of five experiments. The replication rate might be higher or lower, depending on who the experimenter happens to be. There are experimenters (such as myself) who have for years obtained nothing but chance with Schmidt's REGs. There are others, such as Schmidt, who have readily obtained significant results.

On the other hand, I cannot entirely accept Alcock's verdict on the Schmidt (1969b) research. In particular, I do not see any crippling design defects, such as generator bias, that provide an easy explanation for his results. Schmidt's control runs, though not strictly counterbalanced with experimental runs, were extensive, and they were conducted throughout the experiment. These control runs, *even when cut into small segments*, did not exhibit any evidence of short-term bias. Moreover, Schmidt has obtained results when his subjects have been supervised (and they have, I believe, ordinarily been supervised).

One might assume, because R & P say nothing to the contrary, that the other REG studies at least approximate the conditions of Schmidt's research, in that there is some sort of comparison between control and experimental trials. However, there are studies in this data base that include no control trials, and studies that do not even describe the test apparatus.

Over half of the REG data base (188/332) is contributed by the Princeton group (see Radin et al. 1986a), who do describe apparatus and control trials. Their report (Nelson et al. 1984) provides the reader with a great many tables and graphs. However, only four pages of the report are devoted to experimental procedure, and these pages leave many questions unanswered. We do not learn how (or whether) subjects were supervised, or how a subject's preselection of the volitional goal (high-aim, low-aim, or practice) was monitored and verified. It would be wise to defer judgment on these REG experiments until we have a more complete publication.

REG findings would be strengthened by (1) more independent replications or by (2) the participation of critical coexperimenters. Schmidt has himself recognized this and suggested a method whereby critics could, in effect, supervise the research (by assuming responsibility for certain critical control procedures). This method was implemented (though not with skeptical coexperimenters) in Schmidt et al. (1986). R & P argue that that study excluded any fraud or negligence by Schmidt (acting alone). I agree with Alcock (sect. 4.5) that it would be premature to accept that conclusion. Let's wait until the complex design of this experiment has been critically reviewed, and repeated with other coexperimenters.

The ganzfeld studies. Forty-two ganzfeld studies might seem a large data base. However, only 16 of those studies were published in refereed journals. Another 5 were published in a monograph, while half of the studies (21/42) were either unpublished (2/42) or published only as convention papers (19/42).

Not surprisingly, most of the brief convention papers were inadequately reported (Hyman 1985b). Of the 16 journal experiments, 7 were significant by Honorton's (1985) criteria. These were mostly (5/7) from research groups associated with Honorton or W. G. Braud. Sargent's (1980) published work has been another source of encouraging findings. However, the fully reported studies are few, and the methodologically sound studies are still fewer. Hence, I cannot yet see a "strong prima facie case" for a psi anomaly.

A search for the soul? Alcock cites still other problems with the psi ganzfeld data base. He sees, as the fundamental problem, that parapsychologists are not really involved in scientific research. They are involved instead in a pointless "search for the soul" (note Alcock's title). I disagree. The motivations of parapsychologists are generally legitimate, and they are more diverse than Alcock suggests. Alcock seems to be concerned with *unconscious* motivation (rather than stated hypotheses). I doubt whether these motives, assuming that they can be identified, are relevant to the debate.

What sort of motivations or worldviews do parapsychologists have? Some survey results by Allison (1973) are relevant to Alcock's thesis. Allison surveyed the entire membership of the Parapsychological Association in 1972, obtaining a 90% response rate. The survey question of interest (Allison 1973, p. 221) was whether "the results of parapsychological research clearly indicate that there is a nonmaterial basis of life or thought" (Alcock's "secularized soul"?). There was 56% agreement and 43% disagreement (1% had no opinion). The figure of 43% disagreement (with 19% *strongly* disagreeing) seems inconsistent with Alcock's thesis. These respondents must have been either (1) skeptical of the evidence for psi or (2) accepting of the evidence, although rejecting the "nonmaterial" implications. In either case, Alcock's thesis seems to be undermined, at least for a substantial minority of the membership; there is more diversity within the parapsychological community than his stereotype implies.

What about the 56% who endorsed the item? Should we evaluate their research on the basis of what Alcock sees as the "hidden agenda"? Or should we evaluate their research on the basis of stated research hypotheses? If their hypotheses and experimental procedures make sense, there is no need to speculate about a "hidden agenda."

Conclusion. Although I cannot accept Alcock's overall thesis, I do agree with many of his arguments for skepticism. For me, the most telling argument arises in Alcock's discussion of "experimenter effects" (sect. 5.1). The "effect" is really just an observation: Some researchers obtain evidence for psi with relative ease, whereas others (such as myself) never succeed in obtaining such evidence, even after years of research. Some investigators (e.g., Crumbaugh 1958) failed in their search despite using a variety of believers as testing experimenters.

What of those investigators who succeeded? We know from a report by J. B. Rhine himself (Rhine 1974) that a dozen experimenters have produced their results fraudulently. Others have obtained successful results through faulty research techniques. Hence, there is a strong *possibility* that psi arises primarily (or entirely) from experimenter error and fraud. The hypothesis need not imply widespread fraud; the number of researchers who obtain clear-cut evidence for psi is not that large (Akers 1984).

Where is the "anomaly" called psi?

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Rao & Palmer (R & P) are to be commended for the scholarship of their target article. They have provided a reasoned defense of much of the best that parapsychology has to offer. Although there are a number of issues arising from their paper that I would like to discuss, I shall restrict my commentary to some of the more important ones.

1. As R & P themselves admit, it is somewhat contrary to common usage to define psi not in terms of paranormality but only in terms of a lack of adequate conventional explanations of "psychologically meaningful exchanges of information between living organisms and their environment" that "appear to exceed somehow the capacities of the sensory and motor systems as these are presently understood." Although such circumspection is to be welcomed, there are two reasons why I do not think this definition fairly represents the subject matter of parapsychology. First, most parapsychologists, I am sure, are not interested in anomalies per se, but in what R & P call "omegic" influences. Second, parapsychologists do not set out to study anomalies in the way that other researchers do – that is, by attempting to explain puzzling observations made in the course of their research (sometimes leading to serendipitous discoveries, as for example in the case of penicillin). Rather, parapsychologists attempt to *generate* anomalies; for example, they try to show that subjects can succeed in guessing tasks at an above-chance rate. Obviously, this is done with the ultimate goal of demonstrating the existence of omegic psi.

2. I take issue with the claim that experimental parapsychology grew out of the need to account for "people's experiences in the 'real world.'" The early researchers in parapsychology (or "psychical research") were clearly engaged in the search for evidence to substantiate a belief in a nonmaterial aspect of human existence, and they were interested in people's reports because they seemed to give support to such a belief. As I point out in my own target article, most experimental psychologists in those early days who were drawn to the study of paranormal claims abandoned the area because of the lack of evidence that there was anything to study. Furthermore, if one really wants to understand people's experiences, then it is crucial that one first examine all potential psychological and neuropsychological explanations before turning to anomalistic interpretations. I am of the strong opinion that this consideration has been and continues to be lacking in psi research. The psi hypothesis, as Blackmore (1983a) argues, distracts from the development of normal explanations.

3. I am in agreement with R & P about the practical impossibility of a "conclusive" experiment that would demonstrate psi. I disagree with them when they state that if a "conclusive" experiment is more modestly defined as one in which it is highly *improbable* that the result is artifactual, then a case can be made for "conclusive" experiments in parapsychology. My disagreement comes from two sources. First, it is extremely difficult to judge the probability or the improbability of artifact in an experiment, and this is especially true if one has only the written report to go on, because extraneous variables would probably not be reported simply because they were not recognized. In psychology, for example, it is easy to look back at studies done 30 years ago and find sources of artifact that went unrecognized at the time. Second, as I point out in my target article, there are too many "dirty test tubes" (using Akers's [1984] analogy) lying about. R & P do not seem to show much concern about this. An example is the Schmidt (1969b) study discussed both by R & P and by me. Responding to concerns about short-term biases in the random generator that would not have been detected by the longer-term checks that Schmidt reported, R & P cite Schmidt's statement that "many more randomness tests were done than published to satisfy my own questions about the possibility of temporary random generator malfunctions" (Schmidt 1981, p. 41). We are asked to believe, on faith, that Schmidt had eliminated any possibility of nonrandomness in his generator.

4. I cannot accept "statistical replication" as providing meaningful replication. Parapsychologists are trying to demonstrate that an anomaly exists. Their case is weak when based only on slight, albeit statistically significant, departures from chance expectation. That weakness is compounded when the case is defended on the grounds that the number of studies reporting a statistically significant effect is greater than one would expect by chance. Meta-analysis may be a useful tool, but surely not in this instance, when the basic issue is whether or not small statistical departures from chance represent a genuine anomaly.

5. With regard to practical significance, I do agree with R & P that the size of effect is not the crucial factor in determining the practical significance of psi. I also agree with them when they point to the unreliability of psi effects as being more important in this regard. They suggest that such unreliability "probably reflects our lack of understanding of the factors that affect performance on psi tasks." As long as they are willing to agree that these "factors" might all be extraneous variables that produce artifactual results, I cannot disagree. In no way should this unreliability be taken to suggest an "attribute" of omegic psi, such as "elusiveness."

6. With regard to their overall conclusions, I do not share their optimism that they "are indeed on the trail of something interesting." All one needs to presume to account for the data so far adduced is that many psi experiments are not as tightly controlled as is claimed. This view has been attacked as being unfalsifiable, but it is not something that is meant to be an explanation. I submit that in the domain of normal science, it is this very assumption – that other researchers may have made a mistake somewhere along the way – that protects us against error and the development of dogma. Psychologists, for example, are notorious for the way in which they scrutinize one another's research in search of sources of artifact.

After more than a century of research, leading parapsychologists such as R & P can only offer us the possibility of an anomaly, and not necessarily a paranormal one. I continue to believe that it is not anomalies as such, but the search for nonmaterial aspects of existence that lies behind this quest.

Psi and the unwilling suspension of belief

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Alcock provides us with a critical, skeptical analysis of a body of dubious research. Rao & Palmer (R & P) appear to engage in rationalization of belief. This is the essential difference between the two target articles, and an understanding of this difference is necessary to grasp the significance of the arguments made, just as it is a fundamental requirement for the practice of good science. Scientists must be skeptical about the validity of all evidence, and they must be particularly careful, and especially stringent in their analyses, when evidence seems to confirm their own previously held beliefs. They must remain disinterested. Predisposition, particularly when it comes from entrenched belief, is more likely to lead to self-deception and rationalization than to assessment and analysis.

The pernicious effects of predisposition in science were aptly described by a prominent scientist of the mid-twentieth century, Sir Cyril Burt:

> Far more frequent, however, and far more subtle, are the effects of unconscious self-deception – a proclivity which even trained investigators seem at times to underestimate. The tendency to heighten one's statements so as to make them more interesting or enhance one's own importance as the subject of some memorable experience, the desire to avoid qualification or reservations as indicative of an irresolute judgement, and above all perhaps the insistent need to adjust our observations and our recollections to fit our dominant hopes and wishes – these are all ingrained and natural tendencies of the human mind, as unconscious as they are automatic. It needs a long and arduous discipline to turn a man into an exact, objective, and truly scientific reporter. (1967, p. 131)

The real perniciousness of predisposition was even more aptly demonstrated by the behaviour of this same scientist. Burt, of course, engineered one of the best-known scientific frauds of all time by manufacturing test scores to demonstrate what he already "knew" to be true: that intelligence is inherited. Even Burt, who could eloquently describe the dangers of self-deception, was guilty of this very same intellectual crime.

Science has shown us that most of the things we have believed about nature in the past have been dubious, oversimplified, or untrue. Science is the enemy of unsubstantiated and entrenched belief, and just as science undermines such belief, so does belief undermine science. Belief is the antithesis of scientific enquiry because it begs the fundamental question of science – What is truth? – by substituting dogma for truth. Belief implies certainty; science challenges certainty. It is very difficult or impossible to conduct proper science from a position of committed belief. A scientist is engaged in the search for truth; a true believer (unless he is able to suspend belief entirely), ostensibly practicing science, is really engaged in the search for confirmation of what he already knows to be true – he is engaged in rationalizing his belief.

R & P argue that Alcock and others are unfair in suggesting that "investigators who are unsympathetic to psi" should be involved in the replication of psi research, (sect. 4.2.4, para. 1). R & P apparently think that personal beliefs are irrelevant, that positive results from believers ought not to be discounted, and that such criticism is *ad hominem* in nature. They create a dichotomy between believers and those they define as the converse – "disbelievers." The difference they see between these is that whereas one group has been persuaded by evidence, the other has not – yet. They write of experimenters who were probably converted to belief by the strong, positive results they obtained in their first few experiments. Believers, then, are those who have objectively looked at evidence; disbelievers are either those who have not done so or those who are "hardened" and will not do so under any circumstances. The dichotomy is a false one, of course. In addition to believers and disbelievers, there are a great many skeptics who have critically examined the evidence and who remain unpersuaded.

R & P understandably want to describe belief as the mere acknowledgment of evidence, but the poblem is not this kind of belief, which is equivalent to accepting the validity of certain evidence as a working hypothesis. The problem is belief that jeopardizes disinterested analysis and that predetermines the assessment of the evidence and leads to self-deception and rationalization in the interpretation of the evidence. Such belief is indeed harmful to the conduct of science and can, and very frequently does, lead to erroneous or unjustified analyses of experimental data. It is warranted, then, and not unfair to request that experimental data be confirmed by those without prior belief. This is particularly true of data that are used to support claims that are in conflict with the preponderance of existing, well-established data or with logical principles (e.g., that cause must precede effect).

Alcock speculates in an interesting way about the source of belief in psi – that it provides support for the notion of mind–body dualism. We can be sure that this factor alone will cause many people to believe in the existence of psi, and that such a belief will have a healthy life independent of the reality or nonreality of psi. We can also be sure that belief in psi, and belief alone for psychological reasons, would generate an active body of "researchers" busily engaged in confirming their belief. The existence of such pseudoscience should not surprise us, nor of course should the prevailing frivolousness of such activities preclude the possibility of some genuine scientific enquiry in this area, or even the possibility that something like psi exists. But until something really solid comes along, I am with Alcock; I will assume that all of the pother is only an artifact of the very human need to believe.

It is possible, of course, that R & P held no prior beliefs about psi and that they have no predisposition to belief. I have no way of knowing this and wish to make no allegations in this regard. I only wish to point out that their argument in Section 4.2.4 of their target article (on "'Disbelievers' as replicators") is faulty and that the matter of belief is indeed central to the credibility of research reports. In restricting myself to this one criticism, I do not wish to imply that other sections of R & P's article are not similarly flawed; space, rather than opportunity, is my limiting factor.

According to "physical irreversibility," the "paranormal" is not de jure suppressed, but is de facto repressed

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I warmly support Rao & Palmer's (R & P's) presentation of the existing experimental evidence concerning psi. I consider that, far from being excluded by the theoretical formalism of physics, the occurrence of the so-called paranormal phenomena is clearly implied by it.

First, it is now very well understood that physical irreversibility is (in Melhberg's (1961) wording) "factlike, not lawlike." In other words, entropy or probability decrease and advanced waves are, strictly speaking, not suppressed, but strongly repressed at the macroscopic level – somewhat as antiparticles are with respect to particles. Therefore, in either field, appropriate protocols can discern the rare among the trivial.

"Lawlike reversibility and factlike irreversibility" can be stated as follows: In the *de jure* reversible transition

$$\textit{negentropy} \rightleftharpoons \textit{information}$$

the upper arrow prevails in fact over the lower one. So, information as gain in knowledge prevails over information as organizing power. So, in terms of "telegraphing in spacetime," acting in the future and seeing into the past are not compulsory but customary. Precognition is "seeing" into the future, just as acting in the past is psychokinesis. (Incidentally, psychokinesis [PK] is necessarily retro PK: Influencing falling dice means influencing them before the display.)

At its deepest level, the spacetime telegraph is relativistic quantum mechanics. The form of probability calculus prevailing there is the 1926 "wavelike probability calculus" of Born and Jordan; it entails "paradoxical" (i.e., nonclassical) correlations exhibiting what is called "nonseparability" or "nonlocality." It is not inconceivable that in neurophysiology (or even in broader biological domains) this "nonseparability" can show up in the form of liminal phenomena.

In what respect is psi anomalous?

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According to what may justly be called the orthodox, epiphenomenalist view of mind, it is not mental events as such that determine behavior, but brain events. Whether these brain events happen to be accompanied by subjective experiences is of no consequence. Because it would be hard to figure out how electrochemical processes inside the skull could influence the behavior of an electronic random-event generator in another room, it is not surprising that most scientists should be extremely skeptical about claims of this sort. It is all very well for Rao & Palmer (R & P) to say that we must not prejudge the issue, but the policy makers of science will inevitably assign a low priority to a research program that, from their point of view, would at most demonstrate yet again that there are a myriad different ways in which human beings can deceive themselves.

On the other hand, if it *is* a fact that there are people who can bias the output of a random-event generator by mental effort alone, it would be extraordinary if we were not all able to do as much with respect to our own brain. In our daily life we never doubt that our behavior is, indeed, influenced by our wishes and intentions; it is only when we engage in science and philosophy that we even see that there is a problem. What makes parapsychology at once so controversial and so important is that it alone can provide the relevant empirical evidence in deciding between an epiphenomenalist as opposed to a radical dualist (interactionist) position on the mind–brain relationship.

To me, the irony of the present debate is that Alcock, the critic of parapsychology, sees this quite clearly, whereas R & P, the counsels for the defense, fail signally to understand what is truly at stake; hence their dismay that their opponents should persist in refusing to accept their evidence at face value. They talk, hopefully, of "process-oriented research designed to uncover lawful regularities between psi and other psychological or physical variables," but they do not draw the obvious conclusion from the fact that all the examples they discuss relate to psychological variables; they offer no evidence – because there is none – that psi varies systematically with any physical parameter. They admit, with regret, that as yet "no mechanism or theory that would adequately explain psi has been validated," but they give us no hint as to what kind of a mechanism or theory it is that they envisage. Are we, perhaps, to imagine some kind of mediating mechanism between mind and matter? But that would merely be to beg the question as to how such a mechanism could be activated. In the end, it is probably simplest to suppose that the capacity to act teleologically on physical systems, which we see exemplified so strikingly in psi phenomena, is a basic property of minds.

The key question, of course, is: Do we as yet have sufficient reason to think that psi is real? Now, if anything has emerged from the present exchange, it is that this is still, legitimately, a matter of opinion. Personally, I happen to be impressed with the sort of evidence R & P have admirably presented, but then from my philosophical standpoint, it makes sense. Yet, until there is at least one psi phenomenon that can be unequivoçally demonstrated virtually on demand, critics will continue to take refuge in the fallibility that is inseparable from particular cases. Alcock does not need to demolish the evidence R & P present or provide a plausible counterexplanation; nor does he need to accuse anyone of lying or cheating. All he has to do is point to the shortcomings of the experiments in question relative to some methodological ideal.

Whether this evidential stalemate will ever be broken remains to be seen. What can be said already is that what makes psi anomalous is not that it contravenes any known physical law, nor that it transgresses any "basic limiting principle," but simply that it contradicts physicalism – the doctrine, still so widely taken for granted, that everything that happens must ultimately be explainable in terms of physical laws.

Believers, nonbelievers, and the parapsychology debate

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The debate between the sheep (Rao & Palmer; R & P) and the goats (Alcock) is beautifully typified in these two target articles. Although they both describe common literatures on putative psi phenomena (e.g., Schmidt's work; ganzfeld studies; remote-viewing studies, the Maimonides dream research program), predictably different conclusions are reached about what the findings mean. It is as though the reviewers had examined totally different sets of studies. There must be something fundamentally wrong when well-informed and respected scholars disagree so strongly. Or is there?

Alcock suggests that parapsychologists who fail to accept the dismal track record of psi research are blinded by their search for the soul. The real, if implicit, goal of parapsychology is to validate the reality of mind–body dualism using the methods of science. Although I found hints of an openness to dualism in R & P's article, it is equally clear that Alcock espouses a staunch belief in materialism. There is an implication in Alcock's piece that parapsychologists are driven by their metaphysical belief systems and that a considerable amount of variance in their behavior can be explained by these beliefs. Thus, parapsychologists may be seen to conduct flawed experiments, to be taken in by tricksters, to fail to reject the psi hypothesis in the face of disconfirming evidence, and so forth, because their search is not for scientifically validated "truth" but for the soul. All of this may be correct, but are not mainstream scientists also guided and biased by their beliefs?

Mahoney (1976) discussed three consequences of commitment to belief. Those committed to a belief system show a tendency to reason poorly, to be overly emotional, and to arrange their environments so that they do not have to interact with those with whom they disagree. This sounds very much like a caricature of a parapsychologist. Yet, Mahoney was applying this description to "*homo scientus*" (pp. 195–96). A convergence of opinion is forming that this elite group of thinkers is prone to exactly the same types of cognitive biases, emotional reactions, and behaviors as all other people (a group that includes parapsychologists) (Faust 1984; Mahoney 1976; Singer & Benassi 1981; Tweney et al. 1981).

Science has been a successful enterprise because its institutional structure includes a number of mechanisms that attempt to insure that the products of shoddy thinking do not become part of received dogma. As Alcock correctly points out, the scientific arena can be cruel and dogmatic. Parapsychologists do not, however, appear to be any more likely to attack "the messengers because they cannot accept the messages they bear" (Alcock, sect. 6, para. 6) than are scientists in general.

The level of debate between skeptics and parapsychologists would be elevated if the critics would focus more attention on the quality of the evidence parapsychologists present and less on their motives. Alcock criticizes certain parapsychologists for making *ad hoc* arguments. In following the parapsychology debate for a number of years, I have felt that the public declarations of a number of parapsychologists *and* critics have been singularly *ad hoc* and *ad hominem*. Clearly, there have been instances of less than exemplary tactics on both sides of the parapsychology debate. Perhaps a consistently higher standard would be maintained if the debate were carried out in professional journals and forums instead of in the popular and semipopular media as is frequently now the case. If scientists are going to take the time and energy to comment on parapsychological research, then they should do so in refereed journals. In this way, the quality of their criticisms could be evaluated by the peer-review system. In other words, I am merely suggesting that the institutional control to which I referred above be brought to bear on scientists when they speak on the subject of parapsychology. This has occurred, of course, in the present *BBS* format, and there has been a continuing dialogue taking place in the pages of the *Journal of Parapsychology* regarding Hyman's (1985b) and Honorton's (1985) analyses of the ganzfeld literature.

Whatever the ultimate outcome of the parapsychology debate, it is clear that the resolution will not come easily. R & P assert that "one cannot assume that confirmatory evidence [of psi], even from hardened 'disbelievers,' will necessarily be acknowledged as such." Alcock concludes his article with this statement: "All that is needed to turn the attitude of the scientific establishment from doubt to serious interest with regard to psi is to produce some clear, substantive evidence of a psychic phenomenon." If the debate continues on the level at which it has been generally carried out for many decades, we can only expect a maintenance of the status quo. This is why the present *BBS* format and the continuing Hyman/Honorton debate are so noteworthy. It is time for all critics of parapsychology, not just some, to subject their analyses to scientific scrutiny, as they must do in their own specialty fields. Likewise, parapsychologists must strive to meet the methodological and intellectual demands of the scientific arena, as some have done. When critics and parapsychologists begin to play in the same ball game (see Hyman & Honorton 1986), we can begin to reach a consensus, at least at the empirical level. If, after a reasonable amount of time, the parapsychologists do not provide convincing data for communication anomalies, then the scientific community should begin to ignore them as they have done many others who have made unsupportable claims. If, on the other hand, the parapsychologists can document communication anomalies, then everything will take care of itself.

Neuroscience and psi-ence

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Alcock cites the pioneering physiological psychologist Donald Hebb (1978) to the effect that if the psi hypothesis is correct, many empirically supported theories of neurobiology are wrong. Although many neuroscientists and philosophers of mind accept that valid evidence of psi would deal a deathblow to materialist theories of consciousness such as Hebb's, Godbey (1975) disagrees. He argues that psi and psychoneural identity would be logically compatible if the brain/mind could be put in the physical state of "knowing something" by some as yet undiscovered physical conveyor of information. [See also Libet: "Unconscious Cerebral Initiative" *BBS* 8(4) 1985.]

Alcock describes a longstanding schism within parapsychology – between those, like Godbey, who expect new discoveries in the admittedly incomplete physical and neurological sciences eventually to accommodate psi, and those unabashed dualists, attracted to the field because they, like Rhine (1954a), regard it as a bulwark against "dehumanizing" materialist constraints on the mind. The latter must contend with the extensive empirical support for the identity theory (summarized, e.g., by Kandel & Schwartz 1985; Oakely 1985; Shepherd 1983) and its philosophical underpinnings (Bunge 1980; Churchland 1984; Uttal 1978). The former, seeking not to beat but to join neuroscience, face other daunting prospects, which are outlined below.

Virtually all psychobiologists, myself included, espouse psychoneural identity (Uttal 1978, p. 80). For psi proponents, accepting this fundamental tenet of neuroscience adds the burden of suggesting plausible physical, anatomical, and physiological mechanisms for imposing the state of "knowing" on the brain without normal inputs. The peculiarity of such hypothetical mechanisms, and the fact that they are neither required nor predicted by the data of neuroscience, do not preclude them but must certainly dictate caution.

Rao & Palmer (R & P) state that "evidence in science is a matter of degree . . . [and] must be assessed . . . in relation to the plausibility of and the empirical support for the competing hypotheses." For psychobiologists, plausibility of psi versus artifactual explanations for the statistical anomalies of parapsychology rests upon the perceived likelihood of discovering physical and neural interfaces for direct brain-to-brain or object-to-brain communication. Given the lack of independent indications for the existence of such mechanisms, I think it is not as unfair as R & P imply to demand "extraordinary evidence" for psi effects as the only reason to postulate their existence. If evidence for psi were robust and not open to alternative interpretation, we would of course be forced to accept these egregious mechanisms or else abandon the psychoneural identity hypothesis.

Some psi claims, such as postmortem survival or astral projection, are, as Edge (1986, p. 327–47) admits, much harder than others, such as telepathy, to conceive of in nondualistic terms. I shall concentrate, therefore, on prerequisites for telepathy between presumably material minds.

First, the "sender's" brain would need a system to select the appropriate neural substrate of the "message" and encode it in a hypothetical carrier medium suitable for nonmuscular propagation. This mechanism, whose nature and location have not even been hinted at, would then have to transmit the signal. All known brain outputs obey the inverse square law, but this one (though stymied by skeptical influences) is impervious to distance, time, and shielding that excludes all known energies. The informational capacity of this unique transmission medium would have to be immense to convey the complex impressions demanded. A means of preventing crosstalk among simultaneous messages and directing them to the variable whereabouts of one or a subset of roughly 5 billion possible recipients would also be required.

At its distinction, the "message" would, by definition, bypass all normal input pathways to impress itself directly on consciousness, channeled somehow to appropriate brain systems – that is, systems for visual imagery, dreaming, or fear, for instance, rather than for itching or hunger.

Psychophysiology contends that even simple percepts, memories, or emotions involve spatiotemporal integration of activity in millions of widely distributed neurons and their subcellular components. *Extrasensory* perception precludes the activity of peripheral receptors and their afferent pathways that normally determines this central electrochemical configuration. Thus, the hypothetical "psi signal" would have to impress the pattern of synaptic, axonal, and metabolic events directly – by modulating ion gates, exocytosis, refractory periods, and so forth, in millions of neurons in correct sequences and appropriate anatomical pathways. The crudeness of sensations following direct stimulation of cerebral cortex with prosthetic devices (e.g., Dobelle et al. 1974) emphasizes how subtle the neural code allegedly duplicated by psi must be.

Perhaps it was the realization of the magnitude of the burden placed on psi if it must read one brain's neural circuitry and impress it upon that of another that prompted Palmer to speculate that psi might not involve transmission at all – that is, that it might just be the same information simultaneously materializing, acausally, in two places (quoted in Alcock 1981, p. 128).

Several lines of brain research cast doubt on the psi hypothesis (Beyerstein 1987). Consider the well-known effects of commissurotomy (Le Doux et al. 1979). Information confined to one hemisphere in the split-brain patient is clearly inaccessible to the opposite hemisphere. If telepathy can link minds around the globe, why can information not bridge a few millimeters of interrupted tissue in the same brain? Likewise, why does psi not compensate for lost sensory and cognitive faculties in tests of brain-damaged patients (cf. Kolb & Whishaw 1985)?

Alcock alleges that coexisting contradictions within parapsychology make many of its claims nonfalsifiable. A case in point is R & P's "noise reduction model." Despite their potential for noise reduction, it was suggested to Blackmore (1985, p. 428) that her "boring tasks in boring classroom environments" could account for her repeated failures to find psi. Some parapsychologists report that a relaxed, detached, "blank" state of consciousness heightens psi receptivity, but others have found that highly motivated, attentive, and striving subjects perform better. Rhine (quoted in Rao 1966, p. 53) asserts that "exceptionally strong drive is needed for the top-level performance." Stimulant drugs, not known for calming the mind, have reportedly enhanced psi (Rao 1966, p. 33), as have monetary incentives and even punishing psi failures with electric shocks (Nash 1978, p. 107). It should also be noted that the effects of meditation on physiological indices of arousal and attention cited by R & P are not as simple, direct, or replicable as the authors imply (Beyerstein 1985; Holmes 1984; Schuman 1980).

R & P imply that the reluctance of conventional science to embrace parapsychology stems largely from sociocultural prejudice. Most psychobiologists, however, have adopted materialist views of consciousness that are conducive to skepticism about psi on the basis of empirical support for mind–brain identity and *in spite of* "prevailing religious and cultural beliefs, personal experiences and observations, and . . . [the prevailing] world view" that clearly favor dualism and the psi hypothesis. Elsewhere, R & P admit that subjective experiences in the "real world" should suggest to naive percipients that psi is real. Neural identity theory is counterintuitive, held even today by a minority of the public. Neuroscientists accept it because of its parsimony and heuristic value and the lawfulness of the mutually reinforcing experimental, evolutionary, developmental, and clinical evidence (Uttal 1978, p. 80). Convincing as that support seems, however, one must agree with Malcolm (1971) and Uttal (1978) that the ancient mind–body problem cannot be settled empirically. If dualism is correct, neither mind nor psi need conform to materialist constraints.

Alcock is probably right that the psi debate – even between skeptics and psi proponents of the materialist persuasion – is unlikely to be decided empirically because it stems from incommensurate views on the nature and powers of consciousness. Although I laud efforts such as the Hyman/Honorton (1986) collaboration to improve the data base, I suspect that the data will, for this reason, always be open to different interpretation.

Because of the profound implications the existence of psi would have for the neurosciences, I have tried, often with believing students running the experiments, to replicate certain classic parapsychological effects. If our consistent failures were due to "the experimenter effect" [see Rosenthal & Rubin "Interpersonal Expectancy Effects" *BBS* 1(3)1978], I wonder why psi has not helped subjects achieve above-chance discrimination in similar low signal-to-noise ratio tests, not considered by them or by us as psi studies, and where we expected positive results (Douglas 1978). We asked subjects to guess which of several vials of fluid contained a putative pheromone that had no conscious odor. Disappointingly, they selected the vials as randomly as subjects had in our attempted psi replications. Had they shown a small statistical advantage for the "pheromone" vial, how would we have known if it was normal olfactory detection or psi?

Parapsychology's choice

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Alcock has amply demonstrated the way in which parapsychology continues despite its failure to produce convincing evidence for psi. I would like to ask two related questions: (1) Could parapsychology go on forever even without replicable findings? (2) How might the subject progress in the future?

Could parapsychology continue forever? It seems that it could. As Alcock has pointed out, the negative definition of psi and the methods used to explain away failure all help to keep it going. But there is another way of looking at the situation. In most of science – in biology or psychology, for example – progress often occurs very quickly and without one's checking and rechecking every step along the way. Errors due to fraud, carelessness, statistical error, or whatever, undoubtedly occur. They could be detected by meticulous replication, but at a high cost of time and effort. This cost is not justified when effective predictions can be made from theory. Some errors can be tolerated, or, as Alcock puts it, "the truth will out, and error falls by the wayside." This way of conducting science makes sense and is much faster. If all scientific claims had to be strongly replicated before publication, there would be fewer anomalies, but science would progress more slowly and rigidly. Thus, there could be a naturally adopted trade-off between speed of progress and possibility of error. In this way, psi-like anomalies could just be the price we pay for rapid progress, and they will keep being found as long as parapsychologists adopt the heuristics appropriate for the rest of science.

It is noteworthy that parapsychologists often claim, and with justification, to have research methods at least as good as any in psychology. The difference, however, is that psychology can tolerate considerable error and still progress, whereas parapsychology can spend enormous efforts trying to eliminate error and not progress. Of course, this may be nothing more than the jaded view of one who has consistently failed to find evidence for psi over 15 years (Blackmore 1986a). However, it will have some plausibility as long as parapsychology keeps asking the same questions and offering only unreplicable anomalies as answers.

However, parapsychology also claims relevance to many inexplicable experiences. Our present psychology has no remotely satisfactory accounts of mystical experiences, out-of-body experiences, near-death experiences, or visions (mundane or transcendent). People continue to have such experiences and to seek help in understanding them, and they turn to parapsychology for explanations. Here they find some dualist accounts and plenty of use of "psi" as an explanatory concept. But neither of these explanatory ploys works. The experiences remain mysterious, and parapsychology meets the mystery with only a negatively defined, nonexplanatory concept. This it cannot do forever.

How then might parapsychology change? First, the long-hoped-for replicable psi experiment might be found. Then psi could cease to be negatively defined, doubters like myself could be appropriately overruled, and parapsychology could progress with "omegic" theory and hypothesis testing (to use Rao & Palmer's [R & P's] neologism). This is certainly possible, but no one can say how likely it is.

While we await this breakthrough, the alternatives depend entirely on how parapsychologists choose to define their subject area; whether it is confined only to psi or whether it includes those "experiences in the 'real world'" out of which R & P say it grew.

We can certainly hope for scientific accounts of such experiences. Already, progress is being made with near-death experiences (Greyson & Flynn 1984), lucid dreams (Gackenbach & LaBerge, in press), and cognitive approaches to out-of-body experiences (Blackmore 1984; Irwin 1985c) – and all with little or no reference to psi. One could speculate that a deepening and extension of an information-processing account of human experience will lead to something close to views expressed in some religions, especially in Buddhism; or, as Alcock puts it, to finding a secularized soul. For example, we may build on the assumptions that the self is a mental construct, that external reality is nothing more than models of the world, and that consciousness is a natural aspect of information processing. This "cognitive mysticism" could force us to accept our fundamental aloneness, our dependence on the physical body, and the illusory nature of the self and the experienced world – while potentially making sense of higher states and transformations of consciousness in terms of models of reality (Blackmore 1986b). Within this approach, the search for the elusive psi can only be seen as a red herring.

The founders of psychic research were deeply concerned with issues of man's place in the world, the government of the universe, and the nature of human experience and suffering (Gauld 1968). This new approach would not, I imagine, have gone down too well with them, but at least it addresses the issues – which a "science of psi" does not and cannot do. It also provides hope of a truly progressive research program. The stagnant program based on psi (which can never be overthrown by blank skeptical denials) could at last be superseded.

But would this be parapsychology? That is up to parapsychologists to decide. So far, the tendency is to be interested only in the evidence for psi; experiences are studied only if they seem to involve psi or are psi-conducive. In the past, this caused parapsychology to shrink, losing such topics as hypnosis, multiple personality, or animal homing, and retaining only the "still mysterious" ones. If it continues clinging to psi, parapsychology could soon lose all those human experiences that originally motivated the subject. Though parapsychology might never die, its claims of relevance to human experience would be seen as false.

Alternatively, parapsychology could opt to study the experiences, even though they may prove to require no paranormal explanation. This way (and perhaps only this way) the subject could take up a valid and valuable place within science, but it would have to accept that, after all, its subject matter might fit within the "basic limiting principles" (see R & P's Introduction, para. 4) and with the rest of science (unless or until the breakthrough ever came).

So parapsychology has a choice: Give up exclusive dependence on psi and settle for real progress in studying human experience; or stick to psi – hope for the jackpot, but risk eternal stagnation.

How to dismiss evidence without really trying

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In most respects, parapsychologists and skeptics differ in their assessment of the evidence for psi. But they generally agree that if any parapsychological evidence is worthy of serious discussion, it is the quantitative experimental work begun in the United States around 1930. Parapsychology of course had a history before that time, a great deal of which concerned the phenomena of mediumship during the period 1850–1930. But the accepted wisdom, both within and outside parapsychology, is that this earlier body of data – *particularly* the mediumistic material – is inherently less "clean" methodologically and evidentially than laboratory work, and that it can easily be undermined by appeals to the unreliability of human testimony or the possibility of fraud.

This view is completely wrong, however, and it is noteworthy that the majority of those who promulgate it have apparently never studied the mediumistic evidence with any care. Skeptics traditionally either fail to cite or else misrepresent the strongest cases. When it comes to the evidence from mediumship, skeptics tend to generalize from the worst cases. Yet within parapsychology, the strategies are not much different. Most parapsychologists either ignore the mediumistic material altogether or else dismiss it for the weakest reasons. For those parapsychologists who cling (quite naively) to the utility of conventional experimental methods in their own work, this is a convenient gambit; it allows them to appear appropriately tough-minded to their critics.

My view, which I have recently defended at length (Braude 1986), is that the most convincing and important parapsychological evidence *by far* is the evidence from physical mediumship. The best cases of physical mediumship are every bit as clean as, and far more convincing than, the best laboratory experiments. Moreover, they promise to tell us more about the nature of psi than quantitative experiments ever could. These cases easily resist the traditional skeptical charges of error and fraud, and they cannot be undermined by references to other cases that are transparently weak in just the respects in which the best cases are strong.

Alcock's target article (and earlier book, 1981) are disappointingly typical of the skeptic's approach to this material. Both works avoid any mention (much less discussion) of the good cases of mediumship. Yet Alcock quickly rejects the mediumistic evidence, frequently invoking one of the least impressive and most generally irrelevant cases of all – that of Uri Geller. It is an egregious error to attack the mediumistic evidence by citing phenomena that (even if genuine) can be easily reproduced by sleight-of-hand. In fact, no dismissal of mediumship should be taken seriously until it deals, *in detail*, with the cases of D. D. Home and Eusapia Palladino, at the very least (see Braude 1986 for references). In his article, Alcock claims that "[gifted] psychics have as yet been unable to perform their feats under controlled conditions for neutral or skeptical investigators." Yet that is precisely the feature of the Palladino case by virtue of which many consider it to be the strongest in the history of mediumship. Alcock must *show* – not merely allege – that his claim holds for the Palladino case, and especially for the 1908 Naples sittings, which were conducted by three skeptical investigators, two of whom were skilled magicians (Feilding et al. 1909). In fact, I ask Alcock whether he has actually read the primary material (rather than merely accepting the accounts of other poorly informed or confused critics). I challenge Alcock to discredit the evidence from the Palladino and Home cases (as described fully in Braude 1986). Alcock also fails to discuss the careful work over the past few years conducted at the Princeton

University School of Engineering/Applied Science (e.g., Dunne et al. 1983; Nelson et al. 1983; 1984).

I will leave it to experimentalists in parapsychology to reply to Alcock's criticism of their work. I prefer, instead, to focus briefly on problems of other kinds. To begin with, Alcock uncritically accepts the view that psi appears incompatible with received science (particularly the laws of physics). That view is actually quite easy to challenge, however, and Alcock seems unaware of the relevant issues. For example, no evidence available now or in the forseeable future could possibly establish, say, that telepathy violates Maxwell's equations, even if it appears insensitive to distance (see Braude 1986, p. 283; Braude, forthcoming). Moreover, Alcock fails to mention the mediumistic and poltergeist evidence suggesting that PK (psychokinesis) actually obeys conservation laws in physics (e.g., reports of cold breezes preceding physical phenomena, and the measured increase in the weight of certain mediums by the amount of force needed to raise a levitated table; see Braude 1986, Chap. 2). Alcock is also wrong in claiming that it is a "logical" principle that a cause cannot precede its effect. Not only is that principle not a formal truth, but it is also far from obvious (even if true). Indeed, an imposing array of philosophers and scientists consider that the concept of a cause is atemporal, or that the equations of physics are time-reversal invariant.

Alcock's confusions here may be continuous with his apparent failure to grasp important issues about reductionism and dualism. First, Alcock never acknowledges that cognitive or intentional phenomena generally – normal *and* paranormal – might simply lie outside the domain of the physical sciences. Many would argue (quite reasonably) that the methods and theories of physics, for instance, are (respectively) inappropriate and irrelevant to both orthodox psychology and parapsychology. Alcock sees parapsychology as an attempt to defend the existence of a nonmaterial *soul*. Like many other scientists, he apparently does not appreciate the distinction between Cartesian- (or substance) dualism and event- (or level-of-description) dualism. But the nonreducibility of the mental to the physical need not be taken to support the existence of a nonphysical substance, the *mind*. If that were the case, every social or behavioral scientist would have to be as much a closet Cartesian as Alcock supposes every parapsychologist to be. In fact, when it comes to cognitive or intentional phenomena, one can easily be a substance-monist and a level-of-description-dualist. Indeed, there are many different ways to take the position that the only stuff of nature is physical, but that psychological phenomena (normal or paranormal) cannot be countenanced by the physical sciences. The only parapsychological evidence that *could* count against a substance-monism would be the evidence for survival after death, and not even that would specifically support a strong dualism; it could equally support a form of idealism, or even a pluralistic view with an inventory of substance-kinds of at least three (see Braude, forthcoming).

Turning briefly to Rao & Palmer's (R & P's) target article, I was sorry to see them claim that "the first major experimental investigation of psi" was Coover's 1917 study at Stanford, and that "sustained research" began when J. B. Rhine arrived at Duke University in 1927. Apparently, R & P use the terms "experiment" and "research" to apply honorifically only to a certain kind of formal quantitative study. I submit, however, that these terms can be applied without strain to many of the best mediumistic studies. Many of Crookes's studies of D. D. Home (see Braude 1986) deserve to be considered experiments; and sustained research was conducted on Home, Palladino, and other mediums.

More important, conventional experimental methods in parapsychology are powerless to reveal anything interesting about the phenomena, except perhaps that the phenomena exist. There is no way to guarantee that only officially designated subjects use psi, or that subjects use only the psi ability being tested for, or that they use it only at the appointed time. Hence, once we take psi seriously enough to test for it, we give up the ideal of a truly "blind" or "double blind" experimental protocol; there is no way to render an experiment blind for ESP. Neither can we control for sneaky or naughty psi, on the part of the official subject, the experimenter, or even onlookers. No experimental controls can prevent persons even remotely connected with the experiment from using psi to serve their own needs and interests, which (of course) may differ from those of which the subjects are consciously aware, and which are almost certain to be more deeply motivating than the artificial tasks contrived for the subject. Indeed, in the case of PK, the only time we can be reasonably certain who the agent is, is when we have a "superstar" subject in whose presence phenomena occur repeatedly under different conditions and with different experimenters. But, of course, we cannot hope to understand the psychology of psi until we can identify psi agents. That is one conspicuous advantage of the best mediumistic evidence over recent laboratory data, and a reason for resuming the study of mediums. Hence, although I applaud Rao & Palmer's sensible defense of the integrity of experimental work, I can only be pessmimistic about the utility of that body of evidence.

Struggle for reason

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The counterpoint to the Rao & Palmer (R & P) target article has already been provided by the excellent target article of Alcock. The objective of the following commentary (after mentioning some incidental points and complementary material), is to attract attention to a problem whose consequences should be very serious for education, science, and culture.

Concerning the "recognition" of parapsychology, the problem in Europe is a bit different, and in France especially. For example, some parapsychologists (cf. Broch 1985a) claim that there exists an official laboratory directed by Y. Lignon, professor of mathematics at Toulouse University: In fact, Lignon is not a professor, and there is no parapsychology laboratory. Nor are courses in parapsychology taught in French universities: Not a single connection exists between the educational system and parapsychology. To my knowledge, the only official involvement of a French university in the "paranormal" is the telematic service I created for the University of Nice, and this involvement (see below) hardly receives the smile of consent from European parapsychologists!

Concerning Uri Geller, *BBS* readers may be interested to learn that this debunked medium reappeared in Europe in a television broadcast of more than 2 hours ("Droit de Réponse" on March 13, 1987) that was entirely devoted to Geller and his new book. In this broadcast, Eldon Byrd (presented as a major scientist interested in Geller's psi powers) "confirmed" that Geller was tested with nitinol in scientific experiments conducted in U.S. Navy laboratories (*BBS* readers should consult the hilarious Isis center story in Gardner 1981). The behavior of some parapsychologists and mediums seems to depend on which country they are in.

In slight contrast with Alcock, I hold that acupuncture should be considered a superstition because, even in its capacity to produce limited pain relief, its basis seems to be correlation and not causality; Numerous controlled trials have shown that the claims for acupuncture have no scientific validity (Skrabanek 1984). More generally, medicine is currently confronted with a revival of belief in magic, a trend whose implications may be harmful (Broch, in press).

To complement Alcock's point about the failure to attempt to meet LeBon's stringent test, we can add that the situation is currently unchanged. For example, although the $85,000 reward offered is large enough, not a single medium has yet accepted the challenge to give evidence of his alleged powers under scientifically controlled conditions.

Cheating by experimenters, or misrepresentations (like Byrd's) in reporting the circumstances of experiments, is, in my opinion, a *major* source of significant results in favor of parapsychology. It is a pity that this source of highly significant results (cf. Soal, Levy, Tenhaeff, etc.) is absent from R & P's list. This is not an *ad hominem* argument: It is a basic argument about the "methodology" used by parapsychologists in general (see my supported demonstration in Broch 1985a).

R & P's conclusion that "we are indeed on the trail of something interesting" is by now more than a hundred years old. The real search of parapsychology is, as Alcock has suggested, the search for the soul. And even if the compass of the paranormal and the alleged magnitude of these phenomena are decreasing, we cannot conclude that the number of believers is decreasing in the same manner. On the contrary, everyday experience gives evidence that belief is increasing. A study I did in 1982–83 on a group of 120 students in the first stage of scientific studies at the University of Nice yielded the following striking results: The "dilation of time" (relativity) was considered by more than 50% of these students as a "pure theoretical *speculation*," whereas "spoon bending by mental power alone" was assumed to be a "scientific *fact*" by about 68% of the students.

The principal influence of the proponents of the paranormal is to contribute to a mystification of knowledge, inducing a stratified ordering of the world in which a lot of phenomena are irremediably beyond the understanding, and hence the control, of the majority of individuals. The educational system must play a large role in counteracting this diffusion of responsibility. One of its strong priorities must be prophylaxis against the pseudosciences (Broch 1985b).[1]

NOTE

1. With this aim in view, here is a concrete example of what can be done:

The University of Nice became, in November 1986, the first "ghostbuster university" with the opening of a telematic service on the French Minitel system (phones with a monitor-like screen connected to one another via a large computer). This service – "Les Dossiers Scientifiques du Paranormal et de l'Occulte" – provides "zetetic" information on paranormal and occult phenomena. Anyone, anywhere who has access to a Minitel or videotext terminal (Teletel norm) and calls 36.15 (from France, and 33.36.43.15.15 from elsewhere) followed by the Minitel access code ZET can reach the University of Nice and its service ZET – the only service of this kind in France or any other country. The data base consists of about 3,000 videotext pages. It provides zetetic information on all aspects of the paranormal in numerous scientific areas (archeological mysteries, astrology, parapsychology, the supernatural, magic medicine, UFOs, etc.). Users can ask the service all the questions they want, discuss "paranormal" subjects (in real or deferred time), attempt to meet the Broch–Majax challenge – set to all claimants of paranormal powers by Gérard Majax, a magician, and by me, a physicist, with an independent reward of about $85,000 (offered by a group initiated by J. Theodor of the University of Brussels) – and search for various zetetic addresses and texts in a zetetic bibliography of more than 700 references.

Parapsychology on the couch

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Gee, Doc. So you think that's it. All this time I've been thinking it's the experiments and the data, but you think I've really been searching for a soul. Thanks, Doc. I feel better already.

Faced with the observation that for over a century scientists from a wide variety of disciplines – including many Nobel laureates, Fellows of the Royal Society, and other eminent individuals – have studied and contributed to the field of parapsychology, Alcock advances a hypothesis to account for what appears to him to be an anomalous situation. According to Alcock, parapsychology is really a thinly disguised search for a metaphysical ideal, and not really a science at all. To support this claim, Alcock cites the well-known historical antecedents of parapsychology and the opinions of a few parapsychologists, but the weight of his argument clearly rests on yet another *claim* (as opposed to evidence). Alcock's supporting claim is that parapsychology has completely failed to produce any evidence that there is a phenomenon to study.

Parapsychology has produced many hundreds of experiments over the decades, so how does Alcock support his claim that the field has failed to produce any creditable evidence? He does this chiefly by alleging, largely on the basis of the opinions of others, that all parapsychologists are incompetent experimenters incapable of conducting methodologically sound research.

At the end of Section 3.1, Alcock shows himself to be under an illusion that flaw-free experiments are easy to do and are common in the social sciences. This naive notion has been seriously upset by investigations such as those of Wolin (1962) and Barber (1976), and has been finally put to rest by the Peters and Ceci (1982) study. In this last investigation, as *BBS* readers know, 12 papers that had already been published in 12 "highly regarded and widely read American psychology journals" (Peters & Ceci 1982, p. 187) were covertly resubmitted with fictitious names and institutions; 8 of the 9 previously published papers that went through the refereeing process a second time were rejected, and in many cases the reasons given were "serious methodological flaws."

Perfect, unflawed research exists only as an ideal to which scientists aspire. It is not found easily. Given the motivation and a little time, it is fairly easy to spot flaws, or "potential flaws," in any research. Any scientist who has had papers refereed by unsympathetic colleagues or (let's be frank) has refereed papers supporting a rival hypothesis knows this. The point is, do the so-called flaws, no matter how trivial or arcane, justify discarding the experiment and discounting its evidence? Alcock argues "yes" because, following Hyman (1985b), he feels "they are symptomatic of lax research standards" (sect. 3.1, para. 3). If that is the case, then all of psychology is in deep trouble. I think, however, that most scientists, particularly those who have experience in submitting research to exacting journals, would find it hard to agree that an isolated flaw (or in many cases simply an alleged flaw) necessarily invalidates an entire piece of research.

Charging incompetence on such a grand scale is not to be taken lightly, and one has a right to expect strong evidence. Alcock dismisses no less than 241 PK (psychokinesis) experiments by citing Akers (1984), who in turn cites May et al. (1980) to the effect that none of the studies had been properly designed and reported. But May et al. included a number of criteria that were related not so much to methodological quality as to the completeness of the published report; and, *contrary to Alcock's inference*, they concluded that "the bulk of the experiments were sound enough to warrant a major effort at replication" (May et al. 1984, p. 3).

In a similar manner, Alcock relies on Hyman's (1985b) analysis to cast doubt on the psi ganzfeld research, but he mentions only grudgingly that Honorton's (1985) equally thorough analysis directly counters Hyman's conclusions. Given this analytical stalemate – although even some critics (e.g., Scott 1986) have conceded the upper hand to Honorton – few would disagree with Alcock's advice that "any conclusion about a psi ganzfeld effect must await future research." The point Alcock misses is that this applies equally well to the conclusion that there is no phenomenon to be explained.

Finally, Alcock attempts to undermine the work of Schmidt with the sweeping generalization that "little of Schmidt's research is free from serious methodological shortcomings" (sect. 4.5, para. 3), which he bases on Hansel (1980; 1981) and Hyman (1981). In fact, Hansel examines only the first *two* of Schmidt's experiments, and Hyman does not offer a detailed analysis of Schmidt's work, which he finds "the most challenging ever to confront critics" (Hyman 1981, p. 34), but merely counsels a wait-and-see approach. Moreover, it has been repeatedly demonstrated that Hansel's analyses of parapsychological research are fraught with small errors, contain many gross errors of fact, and even show evidence of deliberate distortions (Honorton 1967; 1981; Medhurst 1968; Stevenson 1967). Alcock, who claims to be concerned about lax research standards, apparently sees no contradiction in using a demonstrably unreliable source to argue that another scientist's work is unreliable.

An examination of the evidence that Alcock uses to support his charge of widespread incompetence reveals it to be anything but conclusive. Yet, that curious observation remains: Many scientists, including quite a few whose credentials in more orthodox fields are unassailable, continue to read the same parapsychological reports that Alcock does, continue to study the same findings, yet come to the conclusion that there *is* something to be investigated. Can we accept Alcock's hypothesis that the only way to account for this behavior is to assume that all these scientists are secretly longing to prove the existence of a soul? No. Alcock's supporting evidence is too badly flawed, and we must fall back on the null hypothesis of no difference. These scientists take parapsychology seriously for the very same reasons most other scientists choose their problems – they find the data intriguing and the problems challenging.

Why parapsychology cannot become a science

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The thrust of Alcock's brilliant indictment of parapsychology is that after more than one century of research it has failed to come up with even one bit of hard evidence for the existence of telepathy, precognition, telekinesis, or any other alleged paranormal phenomena. This is quite true, but it may not convince the dyed-in-the-wool empiricist who thinks that future research, with finer experimental methods, might establish the reality of some of the alleged parapsychological phenomena. After all, he may reason, physicists have failed to detect any gravitational waves even half a century after they were predicted by Einstein and his coworkers. Why could psi waves not be parallel?

We consequently need a stronger argument than mere lack of positive evidence. There are at least four arguments against the empiricist who is prepared to wait for another century, and even to have some of his tax money invested in parapsychological research.

A first argument is that the great majority of parapsychologists reject the possibility that they are dealing with normal, though presently unknown, processes that will eventually be explained by physics or psychology: They insist on the paranormality of the phenomena, as well as on the exceptional character of their discipline. For example, they claim that, unlike physical interactions, which decay with distance, the psi phenomena are distance-independent. Thus, two psychics might be able to chat with each other just as easily across the Atlantic as across a table. In other words, parapsychologists are not after physical explanations of the paranormal. In fact, they do not attempt to explain anything, but limit themselves to asserting the existence of phenomena that are not being investigated by normal science and cannot be hoped to be understood within the framework of "normal" science. Theirs would be the anomalous study of the anomalous. Hence, both believers in ESP and skeptics agree at least on this point: that there is nothing to be expected from a scientific investigation of psi phenomena.

A second argument is that the alleged psi phenomena are not just beyond the facts studied by present-day science: They are inconsistent with certain basic findings of science. For example, telekinesis is incompatible with the various laws of conservation of energy, linear momentum, and angular momentum. Indeed, if immaterial mind could move material things at a distance, then energy would be created out of nothing. (In this case, psychics could take the place of waterfalls and of fossil fuel in moving electrical generators.) And if telepathy were possible, the whole of physiological psychology would be false, for it rests on the assumption that mental phenomena are brain processes. On this assumption, it is obvious that thought transmission without a material medium is just as impossible as digestion or respiration at a distance (for further arguments along this line, see Bunge 1980; Bunge & Ardila 1987).

A third argument was unwittingly supplied by Broad (1949), a philosopher who believed firmly in parapsychology. He noted that parapsychology violates certain basic "limiting principles" of all the sciences. For example, precognition involves an inversion of the causal relation, as effects would precede and produce their causes. Moreover, if the nonexistent future could influence the present, nothingness would be causally efficient. Something similar happens with the other kinds of alleged paranormal phenomena: Every one of them violates at least one of the general (though usually tacit) philosophical principles underlying scientific research. One such principle is that the world is composed exclusively of concrete (material) things that behave lawfully – that there are no freely floating immaterial objects, and that if something looks anomalous, it is just because of our ignorance of its laws. (For details on the philosophical background of science, see Bunge 1983.) Oddly enough, even while admitting that such general principles of science are violated by parapsychology, Broad concluded that because ESP was a fact, science ought to give up those principles. But then science as we know it would have to be thrown overboard. Who but antiscientists and pseudoscientists are prepared to pay such a high price for a bunch of ancient superstitions?

A fourth argument is from the systemic nature of science. Every genuine science is a member of a closely knit system of partially overlapping research fields: There are no isolated sciences. On the other hand, parapsychology borrows nothing from other sciences, in particular psychology and neuroscience, and it has contributed nothing to any science. (It makes only some use of mathematical statistics, but statisticians are seldom satisfied with the way parapsychologists handle their science.) Moreover, parapsychologists have usually resisted the suggestion that their discipline become embedded in psychology, let alone in biopsychology. (Psychoanalysis is of course in the same boat.) To put it more concisely: Parapsychology is not a component of the system of the sciences, and most of its practitioners do not wish it to become one. They are more attracted to the ghostly than to the material, and to the mysterious than to what can be explained.

Finally, there are two reasons why the analogy between the putative psi waves and gravitational waves cannot be used to support parapsychological research. First, gravitational waves have been described in exact terms (namely, as solutions to the gravitational field equations), whereas psi waves have only been christened: Nobody knows the equations they satisfy, or how they could be generated or detected. Second, the prediction of the existence of gravitational waves is not a stray conjecture but a member of a solid scientific theory that has been confirmed in minute detail and that coheres with the rest of classical physics. (The hypothesis enjoys the indirect support of about 20 different "effects.") For these reasons, a number of experimenters persist in their efforts to detect these extremely low-energy (hence very elusive) waves. On the other hand, no physicist would be able to design an effective psi-wave detector because, by hypothesis, such "waves" carry no energy – although, in violation of the laws of conservation, they are supposed to cause movements at a distance.

In short, the lack of firm experimental evidence for ESP is not the sole, nor even the most important, feature of parapsychology. What is distinctive about parapsychology, and decisive in rejecting it, is that it is a prime example of a pseudoscience (for more on this, see Bunge 1982; 1985).

Observation versus theory in parapsychology

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The target article by Rao & Palmer (R & P) seems to me an admirable brief account of the experimental evidence for the occurrence of anomalies of the kinds called psi. Extreme condensation may have led at one point to an argument that was impossible to follow; it is not apparent what criterion for "differential scoring" would lead to a chance expectation of 50%. But, otherwise, R & P make a clear case for the presence of anomalies that merit, and are likely to reward, much wider scientific attention than they have thus far received.

The target article by Alcock is valuable in its initial sketch of the status of parapsychology and in its account of various problems that create uncertainty about the genuineness of various apparent anomalies. His account of these methodological problems supplements, with its very different emphasis and conclusions, the account by R & P. Yet his paper seems to me more incomplete than that of R & P. The psi research I know best concerns possible ESP in dreams. Alcock twice mentions my article's (Child 1985) review of that research, without revealing that a main point of the article is that critics (including Alcock) have grossly distorted the most basic facts about that research, so that most people's evaluations of it are based on false information. This serious gap in the information he now offers seems in itself a form of misinformation.

Science develops through the interaction of theory and observation, but individuals differ greatly in the relative strength of their trust in the one or the other. Alcock and some of his fellow critics are dominated by trust in the theoretical aspect of science, and they are led to evaluate parapsychological research very differently from the way they would evaluate other research. Scientific parapsychologists of today are dominated by trust in the observational aspect of science. The founders of parapsychology, as Alcock rightly indicates, were dominated by a specific theoretical concern, hoping their observations would serve to establish mind–body dualism; he is wrong in attributing that concern, in effect, to all modern researchers in parapsychology. Present-day researchers in parapsychology are, as I see them, more often dominated by the orientation of science toward observational fact; they regard their observations as a challenge to present theory, but they are not necessarily committed to any substitute theory. Until their observations are better tied to testable theory, their work is of greater interest to the fact-trusting than to the theory-trusting among other scientists. But the fact-trusting need direct familiarity with the experimental studies. In different ways, both Alcock's article and that of R & P may lead fact-trusting scientists to look for themselves at some of the research. Can the case for psi be as weak as the theory-trusting Alcock claims? Can it be as strong as the observation-trusting R & P claim? Only direct acquaintance with the research can justify a conclusion. The conclusion to which I am led is essentially that reached by R & P; this conclusion would obviously not be shared by all who looked at the evidence, but I suspect it would be shared by a good many.

Differentiating between the statistical and substantive significance of ESP phenomena: Delta, kappa, psi, phi, or it's not all Greek to me

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Rao & Palmer (R & P) consider a number of major criticisms of ESP or psi research as it has been conducted over the past 50 years beginning with the J. B. Rhine (1934) monograph *Extrasensory perception.* Critics have charged the following: "loose" experimental designs in the sense of a lack of proper controls; inadequate or invalid statistical analyses; lack of a "battle-tested" psi experiment; lack of reproducibility of given psi experiments; lack of a coherent foundation from which to understand and interpret the meaning of each claim for ESP; lack of a set of "lawful relationships" to explain psi phenomena; the potential role of deciet, trickery, or fraud on the part of the subject, experimenter, or both; and the possible nonrandomness of random number generators used in ESP research. R & P respond to each of these issues with reasonable arguments, and then proceed to present the results of a number of experiments (some in more detail than others) to demonstrate what they believe to be convincing evidence of the existence of ESP or psi phenomena.

In my opinion, however, R & P neglect to discuss an exceedingly important issue – namely, the development of a set of criteria for differentiating between statistical and substantive, practical, or clinical significance. This becomes crucial because it is well known that even the most trivial of results will reach very high levels of statistical significance whenever the number of cases on which the statistical test is based is large enough. Thus, for example, a correlation of only .01 (essentially zero order) will be significant at the .05 level if it is based on 40,000 cases (e.g., Kazdin 1980, p. 359). Likewise, only trivial average differences between groups will be highly significant statistically when the number of cases (N) per group is sufficiently large. Given the existence of this phenomenon, which holds generally across specific and appropriate statistical indices, a number of prominent investigators have criticized the failure of scientists to differentiate between statistical and clinical significance (Bakan 1966; Barlow et al. 1984; Carver 1978; Cronbach 1975; Cronbach & Snow 1977; Tyler 1931). For example, Barlow et al. (1984) note that "the important issue in this controversy is the size of the experimental effect with which one is dealing, since statistical significance, even when properly interpreted, bears no relation to the importance of or the size of the effect" (p. 27).

A proposed model for assessing magnitude of psi effect (delta). Relating this problem to the corpus of psi or ESP research, one asks whether there is an approach that can be found that does, in fact, treat any given psi phenomenon in the context of the "size of the effect" (delta) above and beyond levels of statistical significance. Do we have an existing statistical methodology that can be applied in order to inform the scientific community about whether a given ESP result is worth talking about? I believe we do. In this very context, Wackerly et al. (1978) and Wackerly and Robinson (1983) discuss kappa-type statistics in the context of testing agreement between a judge and a known standard. It is of interest that as one application Wackerly and Robinson (1983) mention the following: "A self-professed telepathist could be presented with a set of identically backed cards, each with one of four geometric shapes on the side invisible to the judge, with the task of identifying (without looking, of course) those cards which correspond to the various shapes" (p. 183).

It is informative how closely one of the experiments described by Schmidt (1969b) and reported in some detail by R & P fits this paradigm. I am referring to the lamp-lighting experiment in which each of three preselected subjects was given the task of pushing one of four randomly presented buttons on each of numerous given trials. Whenever the correct button was pushed, a light would illuminate a lamp. Fortunately, R & P provide enough detail to make it possible later to apply our proposed model to this ESP experiment.

A statistic that treats data of this type at the levels of both statistical and clinical significance is kappa (Cohen 1960; Fleiss 1981; Fleiss et al. 1969), which can be defined as:

$$\text{Kappa} = (\text{PO} - \text{PC})/(1 - \text{PC})$$

in which PO refers to the proportion of agreement obtained and PC refers to the proportion of agreement expected on the basis of chance alone. Thus, kappa concerns itself with the difference between observed and expected levels of agreement relative to the maximum difference possible. This becomes 1 − PC, because the maximum value of PO is 1. Note further that if PO exceeds chance, then kappa is positive; if PO and PC are equal, then kappa is 0; and when PC exceeds PO, then kappa has a negative value.

Assessing statistical significance of psi phenomena. In order to interpret the level of statistical significance of kappa, one divides kappa by its standard error (e.g., formula in Fleiss 1981, p. 219). The result is a Z score that is readily interpretable by referring to a table of areas under the normal curve. (The validity of this procedure has been theoretically and empirically verified; Cicchetti 1981; Cicchetti & Fleiss 1977; Fleiss et al. 1969.)

Assessing clinical significance of psi phenomena. Because of the aforementioned fact that even trivial levels of agreement will reach high levels of statistical significance provided only that the sample sizes are large enough, a number of statistical investigators have suggested guidelines for interpreting the level of clinical significance of a given kappa value or other measure of agreement or association (Burdock et al. 1963; Horwitz et al. 1984; Kraemer 1981; Landis & Koch 1977). These levels already assume a statistically significant level of agreement at or beyond the .05 level, and have recently been simplified by Cicchetti and Sparrow (1981) and Fleiss (1981) as follows: A value of kappa that is: < .40 = Poor; .40 − .59 = Fair; .60 − .74 = Good; and ≥ .75 = Excellent.

Applying kappa to Schmidt's lamp-lighting experiment. In the Schmidt experiment (1969), a hit rate of 16,458 is reported, over 63,066 trials, summed over three preselected subjects. This hit rate is 691.5 "more than mean chance expectation (MCE)." As R & P note, "the probability that such a result occurred by chance is smaller than 2×10^{-9}." If one assumes that the experiment is valid, then the reported level of probability meets the criterion of a result far exceeding chance. But how about the question of clinical significance or "size of effect"? In order to begin to understand this phenomenon, we have to transform the excess over the MCE value (or 691.5 hits) into an observed percentage of hits to be compared to the percentage of hits expected by chance alone, a simple procedure that R & P neglect to undertake. Because there were four lamps that were lit on a random basis over each of 63,066 trials, we would expect .25 × 63,066, or 15,766.5, hits on the basis of chance alone. The observed hit rate was 691.5 above this, or 16,458. This translates into an observed percentage of correct hits of 26.10% (or 16,458/63,066). Thus, the size of our effect (delta) can be conceptualized as 1.1% above MCE, compared to the 75% that would occur if the hit rate were perfect (100% minus the 25% expected by chance alone).

Substituting the appropriate numbers into the formula for kappa, we obtain:

$$\begin{aligned}\text{Kappa} &= (\text{PO} - \text{PC})/(1 - \text{PC}) \\ &= (.2610 - .2500)/(1 - .2500) \\ &= .0146196 \\ &= .01\end{aligned}$$

Given a standard error of kappa of .0015748 (formula in Fleiss 1981, p. 219), we achieve a Z of kappa of 6.35, which gives us the same level of significance reported by R & P of "smaller than 2×10^{-9}." This is because of the mathematical equivalence of (1) the Z of kappa, the Z produced by the binomial test of a single proportion, and (2) the square root of chi squared-for-a-single-sample, *under the conditions of equally probable chance usage of available categories,* which seems to fit the typical experimental paradigm used in psi research. (When this is not the case, see Wackerly et al., 1978, and Wackerly & Robinson, 1983, for other appropriate statistical models.) Thus, using the kappa approach, we will obtain the exact same level of statistical probability as we would obtain using more traditional statistical approaches. Also, under the just mentioned equally probable category usage, kappa = phi (the familiar correlation coefficient for dichotomous data, i.e., see Cohen 1960, p. 43).

In interpreting the substantive or clinical significance of kappa, it should be stressed that when the 691.5 "excessive" hits are translated into an actual percentage and then are chance-corrected, the result is a chance-corrected size effect of .01, which is at the lowest nonzero level that it is possible to attain. Thus, we have a highly significant statistical result, which is of utterly trivial consequence from a clinical or substantive point of view. In fact, it is analogous to the aforementioned case of the zero-order correlation (also of the magnitude of .01) reported by Kazdin (1980), which reached a level of high statistical significance simply because it was based on 40,000 cases. Another important point to understand about this and a follow-up Schmidt experiment is that the data are not presented separately for each subject (which would seem to be the appropriate way to do it), but are instead averaged over the three participating subjects. One needs to ask what happens when the results are not combined. Does the psi phenomenon (admittedly almost nonexistent) become even less impressive when evaluated on a subject-by-subject basis (e.g., does it disappear for one or more subjects)? One also needs to ask just what sense can be made of a psi effect summed across two or more persons? R & P's response that the purpose of psi research was "to determine not whether a given subject had ESP, but whether the experiment as a whole provide[s] evidence for ESP" (sect. 4.1) is not convincing. I simply do not know what such a statement means.

In addition to the major issues just delineated, there is the question concerning the tests of the randomness of the random event generators (REGs) used in psi research. R & P write the following about the REG used in Schmidt's experiments: "The REG was extensively tested in control trials and found not to deviate significantly from chance."

There is a potential problem with this approach. First, there is always the possibility that the REG might be functioning on a random basis during the control trials and nonrandomly during the experimental trials. This would favor either greater or lesser than chance functioning of the REG, which, if appropriately taken into account, would influence the chance-corrected size effects of psi phenomena. Although some may argue that this possibility is remote, first, even the rather remote is possible, and, more important, there is a way to completely eliminate the problem. The psi investigator should first test the actual experimental output of the REG against pure randomness. Second, the actual REG experimental output should be tested against each psi subject's hit rate. This way, whether or not the REG is functioning randomly, the results can be validly interpreted. Let me give a hypothetical example. Suppose an REG gave a

Table 1 (Cicchetti). *Proposed standards for interpreting the clinical or substantive significance of a given psi phenomenon for a given subject: The 2-choice situation*

% Level of psi accuracy	Size of chance-corrected effect	Level of clinical significance
0–49	<0	None (below chance)
50	0	None (at chance level)
51–69	.01–.39	Poor
70–79	.40–.59	Fair
80–87	.60–.74	Good
88 and above	.75–1.00	Excellent

Table 3 (Cicchetti). *Proposed standards for interpreting the clinical or substantive significance of a given psi phenomenon for a given subject: The 4-choice situation*

% Level of psi accuracy	Size of chance-corrected effect	Level of clinical significance
0–24	<0	None (below chance)
25	0	None (at chance level)
26–54	.01–.39	Poor
55–69	.40–.59	Fair
70–80	.60–.74	Good
81 and above	.75–1.00	Excellent

25.34% hit rate as compared to the 25% expected by chance and that this occurred over the 63,066 trials reported by Schmidt (1969b). This difference of .34% just reaches statistical significance at the .05 level and would indicate nonrandomness of the REG. One would now compare this result (25.34%) against the 26.10 psi percentage hit rate rather than the theoretically expected 25% hit rate. To the extent, then, that a given REG deviates from randomness in a positive direction, the psi results will be further attenuated, relative to what is reported in the literature.

Given these very serious problems, the success rates of psi experiments have to be considered in the same general light as the Schmidt experiments. Here, the problem is even more serious because R & P's only requirement is that a given experiment show a result significant at the .05 level of probability. Once again, referring to the 63,066 trials of the Schmidt study, we have noted that the minimal amount of difference exceeding 25% that will produce significance at the .05 level when $N = 63{,}066$ is 25.34%, or a mere 0.34% above chance expectation. Ignoring the major criticisms of ESP research that have been mentioned by R & P (that is, accepting that none of them is valid), one still has to ask the question, What can a 25.34% responder (over 63,066 trials) tell us about an ESP phenomenon that a random number generator responding with random variation aroud 25% cannot?

But how can one utilize the information provided by the clinical or substantive criteria for interpreting kappa in order to set minimal or other standards for the success of a psi result in advance of the conduct of any given experiment? This can easily be accomplished by knowing two values in the formula for kappa – namely, the desired size of kappa (e.g., .40–.59 to define a psi phenomenon of moderate or "Fair" size); and the percentage of a given psi subject's agreement or hit rate expected on the basis of chance alone. Since psi research typically bases chance on the very simple model of equal frequency of category usage, the formula for expected agreement or percentage of hits is simply $1/k$ in which k refers to the number of categories of choice available to a given subject. These then become, respectively, for a 2-, 3-, 4-, 5-, 6-, 7-, 8-, 9-, and 10-choice situation, 1/2 (.5), 1/3 (.33), 1/4 (.25, as in the Schmidt 1969b experiment), 1/5 (.20), 1/6 (.167), 1/7 (.143), 1/8 (.125), 1/9 (.111), and 1/10 (.10). By simply substituting the ranges of kappa (desired chance-corrected size effects to define "Poor," "Fair," "Good," or "Excellent" levels of psi responding on the part of any given selected subject), we obtain the values given in Tables 1–9.

As an example of how the values in the tables were derived, let us refer once again to Schmidt's lamp-lighting experiment. Assuming that the REG produced an on-the-average hit rate of 25.0%, what percentage of hits must an ESP subject produce in order to give evidence of a fair level of accuracy? Because the fair range of kappa or "size of effect" ranges between .40 and .59, the corresponding hit rates expressed as percentages can be determined by simple substitution into the formula for kappa, using $1/k$ or 1/4 or 25% as PC. For the lower limit, minimally acceptable hit rate (kappa of .40), we obtain:

$$\text{Kappa} = (\text{PO} - \text{PC})/(1 - \text{PC}) \text{ or}$$
$$.40 = (\text{PO} - .25)/.75$$
$$\text{PO} - .25 = .75\,(.40)$$
$$\text{PO} = 55\%$$

For the upper limit hit rate (kappa of .59), we obtain:

$$.59 = (\text{PO} - .25)/(.75)$$
$$\text{PO} = 69.25\%$$
$$= 69\%$$

Thus, when the number of available choices is 4, the psi hit rates defining fair (or moderate) levels of accuracy will range between 55% and 69%. These minimal figures compare to the 26.1% accuracy actually deriving from the Schmidt lamp-lighting experiment. The figures of 55% and 69% are given in line 4 of Table 3. Thus, it can be seen, more generally, that as the

Table 2 (Cicchetti). *Proposed standards for interpreting the clinical or substantive significance of a given psi phenomenon for a given subject: The 3-choice situation*

% Level of psi accuracy	Size of chance-corrected effect	Level of clinical significance
0–33	<0	None (below chance)
33.33	0	None (at chance level)
34–59	.01–.39	Poor
60–73	.40–.59	Fair
74–83	.60–.74	Good
84 and above	.75–1.00	Excellent

Table 4 (Cicchetti). *Proposed standards for interpreting the clinical or substantive significance of a given psi phenomenon for a given subject: The 5-choice situation*

% Level of psi accuracy	Size of chance-corrected effect	Level of clinical significance
0–19	<0	None (below chance)
20	0	None (at chance level)
21–51	.01–.39	Poor
52–67	.40–.59	Fair
68–79	.60–.74	Good
80 and above	.75–1.00	Excellent

Table 5 (Cicchetti). *Proposed standards for interpreting the clinical or substantive significance of a given psi phenomenon for a given subject: The 6-choice situation*

% Level of psi accuracy	Size of chance-corrected effect	Level of clinical significance
0–16	<0	None (below chance)
16.67	0	None (at chance level)
17–49	.01–.39	Poor
50–66	.40–.59	Fair
67–78	.60–.74	Good
79 and above	.75–1.00	Excellent

Table 7 (Cicchetti). *Proposed standards for interpreting the clinical or substantive significance of a given psi phenomenon for a given subject: The 8-choice situation*

% Level of psi accuracy	Size of chance-corrected effect	Level of clinical significance
0–12	<0	None (below chance)
12.5	0	None (at chance level)
13–47	.01–.39	Poor
48–64	.40–.59	Fair
65–77	.60–.74	Good
78 and above	.75–1.00	Excellent

number of choices available to an ESP subject varies between 2 and 10, the lower and upper limits for a moderate chance-corrected level of accuracy range, respectively, between 47% (lower limit, 10-choice psi experiment) and 79% (upper limit, 2-choice psi experiment). The corresponding ranges of lower and upper limits to define a "Good" ESP subject are between 64% and 87%,) and those defining an "Excellent" ESP subject range between 78% and upwards of 87%.

Although one can disagree with my approach, I would respond that because the standards I recommend are applied, without great debate, to assessing the reliability of other clinical phenomena, why should one not apply these same criteria to evaluating psi research? I would go one step further and strongly recommend that each psi experiment, past, ongoing, or future,) use a specific set of clearly articulated guidelines to differentiate between chance, slightly above chance, and substantively significant psi effects. Once this approach is taken, the larger scientific community will be in a position to decide whether there ever has been a psi phenomenon "worth talking about." My own lower limit of acceptability would be at the "Fair" range (chance-corrected size effect of at least .40, a limit I have used over the years to judge the level of substantive significance of phenomena I have investigated).

Although to date I have not come across a specific set of guidelines to be used for evaluating the substantive significance of putative psi effects, the general issue has been broached in a series of letters published in *Science:* As the first example, Dodge (1984) makes the claim that "psi in various forms has been around for a long time and has already been applied for practical (and not-so-practical) purposes in a number of areas" (p. 440). Padgett and Cody (1984) ask Dodge the questions "In what forms has it been around and where has it been applied" (p. 1014)? Kurtz (1984), speaking on behalf of the Committee for the Scientific Investigation of Claims of the Paranormal, notes the following:

> Although psi devotees continually make this claim, it has never been clearly demonstrated that extrasensory perception has any practical applications nor has it been clearly demonstrated to exist in the laboratory. There is no conclusive evidence that dowsers can locate oil or water or that psychics can help locate missing persons, help investigators achieve success in the financial markets, or effectively contribute to the arms race.
>
> Much has been made of late of remote viewing, telekinesis, and the Ganzfeld experiments, but the alleged results are still inconclusive. Parapsychologists talk of an impending breakthrough, but being on the "verge" of something is not equivalent to having demonstrated its practicality or reality. That is why so many scientists remain skeptical about the entire area. (p. 239)

As a final point, it should be understood that I have chosen the Schmidt lamp-lighting experiment as a point of focus because (a) it is one of those cited by R & P as clearly demonstrating a psi effect, and (b) there was sufficient specific information available to make reanalysis of the data possible. Using this reanalysis of the Schmidt experiment as a guide, R & P should both provide this information to the reader and perform analyses similar to what has been recommended in order to further illuminate the critical distinction between the statistical and substantive or clinical significance of reported psi or ESP phenomena. To do less and still expect the scientific community to be convinced of the validity of psi phenomena represents no more than wishful thinking.

It is a matter of considerable scientific curiosity that although the phenomenon of replicability forms the hallmark of validating scientific claims, there still exists such a strong bias against replication studies. This has been noted by Alcock and by many whose fields of expertise lie outside the natural or physical sciences (e.g., see Armstrong 1982; Denzin 1970; Kerr et al. 1977; Reid et al. 1981; Rowney & Zenisek 1980). Fortunately, there has been a recent resurgence of interest in the topic (e.g., Bernstein 1984; Carsrud 1984; Furchtgott 1984; Garber 1984; Heskin 1984; Sommer & Sommer 1984). Nonetheless, until all fields of scientific inquiry are convinced of the critical need for replication studies – perhaps the main reason why the field of psychophysics has made so much progress over the years – it

Table 6 (Cicchetti). *Proposed standards for interpreting the clinical or substantive significance of a given psi phenomenon for a given subject: The 7-choice situation*

% Level of psi accuracy	Size of chance-corrected effect	Level of clinical significance
0–14	<0	None (below chance)
14.29	0	None (at chance level)
15–48	.01–.39	Poor
49–65	.40–.59	Fair
66–78	.60–.74	Good
79 and above	.75–1.00	Excellent

Table 8 (Cicchetti). *Proposed standards for interpreting the clinical or substantive significance of a given psi phenomenon for a given subject: The 9-choice situation*

% Level of psi accuracy	Size of chance-corrected effect	Level of clinical significance
0–10	<0	None (below chance)
11	0	None (at chance level)
12–46	.01–.39	Poor
47–64	.40–.59	Fair
65–77	.60–.74	Good
78 and above	.75–1.00	Excellent

Table 9 (Cicchetti). *Proposed standards for interpreting the clinical or substantive significance of a given psi phenomenon for a given subject: The 10-choice situation*

% Level of psi accuracy	Size of chance-corrected effect	Level of clinical significance
0–9	<0	None (below chance)
10	0	None (at chance level)
11–45	.01–.39	Poor
46–63	.40–.59	Fair
64–77	.60–.74	Good
78 and above	.75–1.00	Excellent

will remain increasingly easy for parapsychologists to label the phenomenon as one of the "pot calling the kettle black."

Random generators, ganzfelds, analysis, and theory

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1. Conundrums of analyzing the machine output.[1] The 1s and 0s in Table 1 were randomly generated. As I decided before leaving for work, I created four sequences of 40 by beginning on the 16th line of page 3 of Owen's (1962) table of random numbers and entering a 1 whenever the last digit of the number was odd, and a 0 whenever it was even. The Pearson chi-squares I computed for each sequence (also entered in the table) are unsurprising – as are the number of alternations (the number most discrepant from "chance" being 15 out of 39 possible; chi-square = 1.64, N.S.).

Now suppose that a psi subject were to press a button during two of those sequences in order to influence the digits to be 1s. If this subject did so randomly, the probability is 1/6th that he would pick A and B. If I, as an analyst, were to pick the 1s and 0s as my units of analysis (they were "randomly chosen," weren't they?), I would observe 42 1s and 38 0s during the intervals that the subject pressed the button, as opposed to 31 1s and 49 0s during the other two intervals. The Pearson chi-square testing this difference is 3.05, which is significant at the .05 level one-tailed (the standard tail used by psi researchers). But by picking two sequences at random, the subject actually has a one-tailed probability of .17 of obtaining 42 1s and 38 0s, *not* .04.

When Dean Radin graciously agreed to share the data on which he based his 1985 meta-analysis of psi effects with random-number generators (Radin et al. 1985), I was somewhat surprised to find that in 1975 Bierman had conducted three experiments, the last of which contained 101,211,635 trials (!). The unit of measurement was the output of the generators. Given that there will be some variance of output within each of the periods in which the subject either attempted to influence the output or not to influence it (or to influence it in one way or another), it would be better to use these periods as the units. Specifically, work out the probability of choosing *influence periods* with a summed value of hits as high as or higher than particular values (e.g., that actually obtained). Under the null hypothesis of no effect, the theoretical distribution of chi-square (or z) values will be identical – given that the variance of a sum of independent events is the sum of the variances; but because patterns of pulses *actually occur* in periods when the subjects were or were not trying to influence them, such time periods are a more natural unit. Moreover, such an analysis would obviate problems arising from any unknown nonrandom component

Table 1 (Dawes). *Hypothetical data*

A	1 0 0 1 1 0 0 1 1 1 1 1 1 0 0 1 1 1 1 0 0 1 0 1 1 0 1 1 1 0 1 1 0 0 0 0 0 1 1 1
B	1 0 0 0 0 0 1 1 0 0 0 1 1 1 1 1 1 1 0 0 0 0 1 0 1 0 0 0 0 0 0 1 0 1 1 1 1 0 1 0
C	1 1 0 1 0 0 0 0 0 1 0 0 0 1 0 0 0 0 0 0 0 1 0 0 1 1 1 0 0 0 0 1 0 1 1 1 0 0 1 1
D	1 0 0 1 1 0 1 0 0 0 0 0 1 0 0 0 0 1 1 0 0 0 1 1 1 0 0 0 1 0 1 1 1 0 1 0 0 1 0 0

Summary

#1	#0	Chi-square
24	16	1.23 N.S.
18	22	.23 N.S.
15	25	2.03 N.S.
16	24	1.23 N.S.

influencing the output of the machine, because the null hypothesis would now be that the *periods* were chosen at random, and output that was not perfectly random *within* periods would not affect the analysis.

But despite my qualms about data based on pulses, I reanalyzed the Radin et al. hit rates data that formed the basis of their proceedings paper (1985). The number of trials per coded "experiment" (there were 184) ranged from 144 to 101,211,635, with a median of 19,188.5; the skew was very positive, and even taking the square root of the number did not do much to eliminate it.

I was interested in the relationship of the number of trials to the results. First, the overall proportion of hits (counting each experiment equally – i.e., not weighing by number of "trials") was .5056. That is consistent with Rao & Palmer's (R & P's) "weak signal theory." What is *not consistent*, however, is a strong relationship between the magnitude of the results and the sample size. Increasing sample size should simply decrease the variance about the population value of the presumed weak signal; such an increase should not bias the results either toward high or low values. What happens, however, is that there is a strong negative relationship between sample size and magnitude of effect. Given the highly skewed nature of the sample sizes, I computed a phi based on a median split of both variables and obtained a value of −.47. In fact, for those experiments with an above-median sample size, the proportion (again unweighted) of hits was .5003, which – even under the dubious assumption that each experiment reported is independent – is not significantly greater than .50.(t = 1.16). In effect, the weakness of the signal dissipates toward nonexistence under scrutiny. (Note that Hyman [1985b] found the same negative correlation between sample size and size of effect in the ganzfeld experiments – to be discussed next. Honorton's [1985] response was that good experimenters knew in advance that they did not need to run as many trials as did less good experimenters. That explanation does not seem applicable here.) Where sample size does have a predicted relationship – but only if the null hypothesis is accepted – is on the absolute deviation from .50 (r = −.55, again based on dichotomization); in fact, this correlation is more negative than that between the sample size and absolute deviation from the *obtained* ("weak signal"?) mean of .5056 (r = −.29). Strange. (Note that these correlations are not independent.) Having data that specify the subjects' success during each period of influence might help clarify these anomalies.

There may, of course, be an important *qualitative* distinction between the large n and the "small" n ($<$ 19,000) studies reported by Radin et al. If so, it is a potentially important one. I am not familiar enough with the details of the experiments to speculate about what it might be.

2. Being and nothingness. Honorton's (1985) "direct hits" analysis of the ganzfeld experiments indicates that there would have to be 432 studies in the "file drawer" in order "to raise the cumulative probability of the 28 known studies of a p of .05" (p. 62). The problem is, Why not? People search *post hoc* for "positive results," and for all we know, there are 432, or even 4,320, studies in the file drawer. I reanalyzed the data using an alternative procedure (Dawes et al. 1984). Suppose that all the z values reported by Honorton above the .05 level of significance were simply chosen at random from values above 1.96 from a normal distribtuion with mean 0 and variance 1 (e.g., for journal submission). What would the average and variance of such values be? The answer is 2.34 and .14 (mathematics omitted). Using the six studies that were published in (presumably reviewed) journals that reported z values above 1.96, I obtain an average of 2.85. The variance about 2.34 under the null hypothesis is .14/6. Hence the z evaluating departure from 2.34 is $.51/(.0233)^{1/2} = 3.34$.

This analysis also gives credence to Honorton's conclusion that there is not nothing there (or – to state the logic of null hypothesis testing more precisely – that there is nothing there, we obtained an unusual value of it). But what is it? Experimental "leakage?" Selective termination? Unconsciously biased coding? Cheating?

I did not perform a similar analysis on the pulse data due to the problems raised in section 1 of this commentary.

3. N-rays. The problem is that these data are virtually impossible to evaluate without a theory that specifies when we would expect *not* to find psi. (R & P's "weak signal" theory doesn't do that.) More conventional theories have floundered for precisely this lack. For example, the discovery of learning – particularly of language – in the absence of any external reinforcement, necessitated the introduction of the concept of "self-reinforcement," which made the circular vacuity of "reinforcement" theory transparent (at least insofar as the "law of effect" postulated that such reinforcement was *necessary and automatic*).

If, in contrast, someone were to develop a theory that psi should occur under conditions X and not occur under conditions Y, then that theory at least could be tested. Blondlot and his colleagues could observe N-rays at their laboratory at the University of Nancy. Others could not at their laboratories. When the American physicist Robert Wood visited the laboratory in 1904, he was shown an experiment in which an observer from the laboratory could see the fluorescent hands of a clock become enhanced by N-rays. The experiment required a darkened room, and among other manipulations, Wood surreptitiously removed from Blondlot's apparatus an aluminum prism necessary for the production of these rays. The observer still noted the effect (Klotz 1980). If he had not, that would have been evidence for the existence of N-rays. That is the type of test that is needed: an experimental control of the *conditions* supposed to lead to the existence (or at least the magnitude) of psi, perhaps one about which the experimenter is "blind." Such a test is different from randomizing choices within an experiment, run by committed experimenters. That test would be more desirable than more discussion and argument about amalgamating statistical results.

NOTE

1. Dean Radin is well aware of the issues and findings covered in this section, as I learned through personal contact (5/30/87) with Radin after writing it. I am including this section anyway, because Rao & Palmer do not cover the material in it. Radin, in analyzing his own data, uses "effort periods," as well as pulses, as units of analysis. I suggest that the truncated normal analysis outlined in section 2 of this commentary could be applied to z scores based on these units. The problem raised in section 3 concerning existence versus nonexistence as opposed to the conditions of existence still remains.

Orthodoxy and excommunication in science

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Follow if you will, in these pages, the torments of a religious conscience as it confronts heresy. The heretic's thoughts and works appear to be steeped in blasphemy, and that the heretic and the orthodox believer might profess the same religion is an idea simply too painful for the orthodox imagination.

Alcock, the orthodox believer, anathematizes the parapsychological heretics and casts them out among the damned: "Psi phenomena are defined implicitly in terms of their incompatibility with the contemporary scientific worldview"; and "the concept of paranormality implicitly involves *mind–body dualism* . . . , the idea that mental processes cannot be reduced to physical processes and that the mind, or part of it, is nonphysical in nature."

Should the heretics, anathematized, be excommunicated from Science? According to Alcock, yes: "The search for psi is now, as it has been . . . , the quest to establish the reality of . . . some form of secularized soul" and is therefore not science. But Alcock, a generous inquisitor, offers parapsychologists the opportunity to reaffirm their orthodoxy: They will be readmitted to the faith if they "focus on the anomalies while putting the concept of psi aside."

Alcock's orthodoxy is so narrow as itself to define a cult within the ecumenical establishment of science. Alcock postulates an a priori materialistic nature that precludes the very existence of phenomena of the kind reported by parapsychologists. This kind of reasoning has been seen in psychology before. Behaviorists adopted some of the scientific philosophy of the Vienna Circle as a justification for constructing a restricted set of axioms both necessary and sufficient to explain behavioral data (Koch 1981, pp. 262–66). The advantages of this approach to science are immediate: The psychologist with a small set of axioms is spared uncertainty about what constitutes behavior (S–R [stimulus-response] associations) and what constitutes an explanation of behavior (a formal S–R theory). These restrictions allow the free play of imagination over a narrow range of possibilities – the intellectual equivalent of playing in a sandbox.

Alcock defines science to match his own orthodoxy, while setting parapsychology outside science as nonmaterialist and dualist. Spiritualism and dualism are explanatory principles that many parapsychologists (e.g., Palmer, as Alcock acknowledges) have no knowledge of or interest in. Nevertheless, Alcock tars parapsychological research with a brush dipped in spiritualism, survival, dowsing, and the aura. None of this is relevant.

Alcock is concerned because psi phenomena do not appear to dovetail with current physical theory. This may be disturbing, but it should not trigger accusations of scientific heresy. Alcock worries that in parapsychology "the hidden agenda is the question of dualism," but he forgets that research motives do not determine scientific facts. The motives of parapsychology researchers vary: So do the motives of any group of scientists. Alcock's worry, taken one step further, would bring into question any research reported by someone who professed a belief in a deity. Religious belief or nonbelief is unrelated to any criterion of research competence, and equally unrelated to the validity of experimental phenomena in parapsychology.

The defining characteristics of science, whatever the subject, are method, phenomena, and results. Explanations come later. Changes in scientific worldviews follow research advances; they do not lead them (Kuhn 1962, pp. 52–65). Anomalous psychological phenomena occur; the people who study these phenomena are called parapsychologists, and their motives for studying the phenomena are irrelevant. What matters is whether their experiments are valid, and what is learned from them. To ignore these simple principles is to forget that science is empirical.

Alcock and Rao & Palmer (R & P) both refer to Hyman's (1981) criticism of parapsychology, which is that each generation has its own favorite candidates for providing evidence demonstrating psi, and that each successive generation finds the earlier evidence passé.

Passé, but not discredited. Passé is a style word and an emotional judgment. The classical evidence from the ESP experiments of Pratt and Pearce (Rhine, J. B. 1964) becomes passé because people no longer remember or pay attention to it, not because it ceases to exist. Attention and memory are variable and fallible, and they are unrelated to the value of the objects or facts that stand in or out of their light. As R & P (and Hyman) observe, evidence for a variety of parapsychological phenomena is accumulating. When the collective professional memory cannot retain the evidence, the fault, as R & P suggest, is not in the evidence, but in ourselves. Two generations of "passé" but reliable evidence for psi phenomena is a lot of evidence.

R & P review evidence for replicated parpsychological phenomena from random-event generator (REG) experiments (sect. 3.1), "sheep–goat" studies (sect. 4.1), differential effect studies (sect. 4.1.3), ganzfeld experiments (sect. 4.1.2), and studies of the remote-viewing phenomenon (sect. 5.6) without once attributing the phenomena to God, reincarnation, the soul, the tarot, or the aura. Alcock builds his argument justifying the scientific excommunication of parapsychology on the premise, which is both false and a nonsequitur, that parapsychology is nonscientific because parapsychology researchers are searching for the soul.

No one can reasonably claim that the principle or principles of parapsychological phenomena are understood. R & P do not claim that. They do defend the validity of several sets of experiments in their article. Between anathemas, Alcock finds some common ground with them in discussing the random-event generator and ganzfeld studies. But it is not the technical quality of parapsychology research that concerns Alcock the most. He criticizes science conducted by people who, according to his explicit argument, pretend to be studying nature when they are really searching for the soul.

There is in reality no danger that parapsychologists are going to divert the wealth or sap the energy of established science. They are not among the fortunate when tithes are distributed to the faithful. It is simply their *presence* in the congregation of science that disturbs Alcock. Why? Because, as he so carefully documents, parapsychological phenomena profoundly disturb the preexisting, "scientific worldview." Parapsychologists are irritating because orthodoxy cannot answer the question: *What if they are right?*

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Parapsychology as a search for the soul: Psi anomalies and dualist research programs

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One of the issues raised by the two target articles is the relationship between parapsychology and dualism. For Alcock, the connection between parapsychology and dualism is close. Rao & Palmer (R & P) define parapsychology as the study of a certain kind of anomaly, that is, psi anomalies, and dualism is not mentioned at all. It is perhaps implied by R & P that parapsychological research may rationally be pursued independently of dualist viewpoints. Alcock seems to be of the opinion that parapsychological research can only persist in the face of the repeated failures to prove the existence of psi insofar as it is fueled by dualist convictions. He also seems to be of the opinion that dualism is unacceptable from a modern scientific point of view.

It should be possible to agree that the kernel of parapsychological research must be the study of apparent psi phenomena – that is, apparent exceptions to the general assumption that information about, or influence on, the environment presupposes sensorimotor interaction with the environment.

There is, of course, no necessary connection between dualism and the study of possible psi anomalies. At the same time, it also seems to me true that there are special connections between parapsychological research and dualist viewpoints – not only historically, but also rationally. Parapsychology makes particularly good sense *if* one accepts a dualist position as a working hypothesis. I also accept dualism as a *sensible* working hypothesis, so that parapsychology to me can be a sensible part of a sensible dualist research program.

Much depends on what is to be understood by "dualism." I assume that a dualist accepts at least the following hypotheses:

(1) The assumption of physical realism: Physical objects and processes exist, in a literal sense (they are not just useful fictions), and they exist independently of any mind.

(2) The assumption of psychological realism: Psychological phenomena also exist, in a literal sense.

(3) The assumption of difference: Psychological phenomena are not identical with any material phenomena.

(4) The assumption of interaction: Psychological phenomena are able to influence, and be influenced by, material phenomena.

Dualism is set apart by (1) from idealist, phenomenalist, and instrumentalist positions. Assumptions (2)–(4) set dualism apart from various materialist positions (eliminative materialism, identity theories, epiphenomenalism). I do not regard (1)–(4) as unreasonable assumptions – particularly if (3) is interpreted as an assertion about mental properties or types, so that it is compatible with token–token identity between mental events and material events. Some of them may of course be false, as implied by various materialist alternatives to dualism, but this I consider as still an open question. It is not easy to formulate a satisfactory materialist theory of mind, as illustrated by the arguments materialists use against other materialists (see, e.g., Block 1980).

Now it is also possible to conceive of various research programs guided by some, or all, of assumptions (1)–(4), or by their negations. A research program where assumptions (1), (2), and (4) are accepted as working hypotheses and the truth of (3) is left an open question could be called a mentalist research program. A research program where (3) is also accepted as a working hypothesis can be called a dualist research program. A dualist research program is a particularly strong version of a mentalist research program. The aim of research is not just to explore what causal properties and relationships may characterize mental states, processes, and activities, but also to explore what may be *uniquely* mental, that is, what cannot also characterize material states (brain states, behavioral states, functional states of the nervous system, etc.).

One may pursue parapsychological research as part of a dualist research program: It is not unreasonable to explore the possibility that an assumption like (3) may find some support through parapsychological research. For example, it might be argued:

(a) If mental events are identical with any material events at all, it is with complex neural events in the central nervous system, for if mental events are to be identical with material events, it must be with material events playing a significant role in controlling and coordinating the behavior of the subject: As far as we know, only complex neural events in the central nervous system can play such a role. In particular, mental states with an intentional content must be identical with neural events sufficiently complex to make a systematic difference to the content of mental states.

(b) What we know about such neural events independently of parapsychological research makes it reasonable to assume that they interact with the environment only in a content-relevant way (i.e., a way making a difference to the content of mental states) through specialized sensory and motor mechanisms.

(c) Mental events involved in psi events do, by definition, interact with the environment in a content-relevant way independently of such specialized sensory or motor mechanisms.

(d) Consequently, if parapsychological research should indicate that psi phenomena really occur, we at least have reasons for believing that mental events involved in these psi phenomena (and presumably other mental events similar to them) are not identical with any material events.

Even if the existence of psi phenomena should be demonstrated (or maybe just *because* it is demonstrated), we may have to revise the opinions expressed in (a) and (b). So there is at most a contingent connection between the existence of psi phenomena and the truth of dualism (cf. Dybvig, forthcoming). This does not mean that (a) and (b) do not express reasonable opinions, however.

What can be said in general terms about the type of contribution parapsychological research might be able to make to dualist theory? Two points seem to be noteworthy:

A. The above argument (a–d) appeals to a causal property of mental events involved in psi phenomena – that is, a property of mental events defined in terms of how they are able to influence and/or be influenced by other events. Such a causal argument contrasts with noncausal arguments often deployed by philosophers in favor of the assumption of difference – that is, arguments where the uniquely mental property appealed to is not a causal property (e.g., the property of having a "raw-feel" sensuous character, or being an object of direct knowledge, or lacking a spatial location). One way of describing the distinctive contribution of parapsychology, as part of a dualist research program, is to say that it promises to supply evidence for causal properties that can be deployed in arguments supporting (3), the assumption of difference. What is important about such causal properties is that they may also be appealed to in support of (2), the assumption of psychological realism, and (4), the assumption of interaction, because they show that mental phenomena are explanatorily indispensable – that is, material phenomena cannot fill the same explanatory role. Parapsychology thus promises to supply particularly strong arguments for a dualist theory, which is defined not just by (3) but by (1), (2), and (4) as well.

B. Parapsychological research as part of dualist research programs typically represents what we may call an externalist rather than an internalist strategy. By an internalist strategy, I mean a strategy in which one seeks to define a uniquely mental causal property in terms of how the mental states of one and the same person interact with one another, or in terms of how the mental states and the material states of one and the same person interact. By an externalist strategy, I mean one in which the causal property is defined in terms of how the mental states of a person interact with the *environment* (including the mental states of other subjects). Now, I think that somebody accepting dualism as a working hypothesis must admit that there is little hope that science (e.g., neurophysiology) will in the foreseeable future supply the dualist with plausible internalist causal arguments in favor of a dualist position. Material events in the human body occur within an enormously complex network of material interactions, making it difficult to verify that mental events in relation to bodily events have causal properties incompatible with these mental events themselves being bodily events. Much better opportunities for control are offered when mental events are studied in relation to external environmental events.

Of course, the externalist strategy that (most) parapsychological research represents would be of little interest if research up to now had shown that it was doomed to failure because psi phenomena do not really occur. R & P's arguments, coupled with what other knowledge I have about parapsychology, make me doubt that this can be true / just as Alcock and other critics of parapsychology have convinced me that the evidence for genuine psi phenomena is still too weak and problematic to allow us to assert that they have been proved (in any sense of "proved" appropriate to an empirical science).

Anomalous phenomena and orthodox science

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It is possible to take three views of anomalous phenomena that seem to go counter to our present-day knowledge of science. We can be credulous and accept these phenomena without detailed examination. We can be skeptical and reject these phenomena without careful scientific investigation. Best of all, we can apply the usual standards of scientific investigation to the phenomena and, on the basis of such an examination, arrive at conclusions that must inevitably be tentative, but that should not go counter to the evidence that is available. As Eysenck and Sargent (1982) have tried to show in their book-length review of the literature, there are now enough well-documented, and even replicated, phenomena in the field that are not subject to the usual criticisms. I will give just a few examples to illustrate what I mean; these are taken from our book, which should be consulted for details.

Most readers will be familiar with the alleged ability of Geller to bend spoons and other objects by stroking. His work has rightly attracted much ridicule. Few readers will be familiar with the work of Crussard and of Bouvaist (see Eysenck & Sargent 1982), two investigators who were funded by a French metals company and who, in their examination of metal-bending effects on any particular strip or rod of metal, took the following experimental measures of the hallmarked samples: (1) All dimensions were measured before and after bending; (2) the microhardness of the metal was measured in several points, again before and after bending; (3) residual strain profiles (measures of crystalline structures) were examined; (4) electron micrograph analyses of the fine structure of ultrathin foil specimens were often made; and (5) analyses of chemical composition at various places along the strip or rod were made. These investigators, as well as John Hasted, a British metallurgist, have reported metal-bending effects that cannot be accounted for by any tricks and that indeed produce effects that defy explanation consistent with metallurgical principles. Thus, these workers have been able to obtain bending of metal rods that would require a force in excess of the physical strength possessed by the human subjects taking part in the experiment.

Some of the effects produced by stroking are physically impossible. Thus, one test used by Hasted employs a brittle alloy bar that cannot be bent to a particular angle of deformation in less than a certain known time: If any excessive force is applied, it simply breaks. The only way to bend it is by applying a small load continuously over time, which produces a bend by a process known as "creep" (which, as the name suggests, is a slow business). Yet Hasted reports bending of such alloys in well under the minimum time thought to be possible using a "creep" process by stroking.

Another apparently "impossible" PK effect is a so-called plastic "deformation." Hasted has reported a case in which a metal-bender gave him a spoon after gently stroking it, and "it was as though the bent part of the spoon was as soft as chewing-gum." Hasted states that "I do not think there can be any question of fraud when the really plastic bend is produced under close scrutiny" (1980, p. 194). Such an effect could occur if corrosive chemicals were applied to the metal, but had they been so applied, they would have discolored the metal and there would also have been a loss in weight due to the corrosion. Hasted weighed the spoon before and after the bending and found no weight loss.

Many other examples could be cited from the work of these metallurgists; perhaps because these results seem so inexplicable and conclusive, they are sledom mentioned by critics of psi. Yet such criticism should be directed at the strongest examples of psi rather than at the weaker ones.

We give many other examples in our book of strong evidence for psi, usually disregarded by critics. We also take up a point often made in complaining that there is no theory and, thus, no prediction in this field. This is clearly untrue. I made the prediction many years ago that extroverts should do better on psi tests than introverts, for reasons that were based partly on a theory of extroversion/introversion, partly on the theory of lower cortical control favouring psi. The results, as surveyed in our book, have been very positive, demonstrating that predictions can be made and verified by tests in this field.

It is true that there is no general theory underlying these phenomena, but neither is there any general theory for most psychological phenomena, nor even for most physical ones. Gravitation, 300 years after Newton, still has two quite different theories to explain it, namely the graviton theory of particle interaction of quantum mechanics and the relativistic theory of space–time distortion. Why should psi do better than gravitation in this respect? I conclude that although skepticism is always called for, one should be skeptical of skeptics who do not properly survey the literature and who do not discuss the strongest evidence for psi, and whose conclusions seem to be based more on prejudice than on fact.

Anthropology and psi

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Although it might appear strange that an anthropologist has been asked to comment on these two target articles about the paranormal, anthropology has not been uninvolved in the psi debate. Margaret Mead played no small role in obtaining membership status for the Parapsychological Association in the American Association for the Advancement of Science in 1969 (Howard 1984). More recently, a paper was published in anthropology's own well-respected peer commentary journal *Current Anthropology* [on which *BBS* is modeled], wherein the author suggests that magic, sorcery, and witchcraft, as practiced by primitive people, might be best explained as reflecting genuine psi phenomena (Winkleman 1982). Here, the admirable philosophy of cultural relativism that permeates anthropology (cultures should be judged within their own contexts) has been raised to the status of empirical acceptance (when a shaman says the spirit of the raven has entered his body, that is exactly what has happened).

Beyond this, even in anthropological archaeology, it has been suggested that psi skills can be used to find archaeological sites, including shipwrecks, prehistoric sites buried by geological processes, and lost cities (Goodman 1977; Jones 1979; Schwartz 1978). Once sites are found, these same authors suggest, they can be most effectively excavated under the direction of "sensitives," and they can be meaningfully interpreted by individuals who can produce what amounts to a prehistoric ethnography derived from the "vibrations" given off by the recovered artifacts. As one involved in skeptically assessing the validity of psi claims related to archeaology (Feder 1980), I was quite interested to read Alcock's and Rao & Palmer's (R & P's) target articles.

I find myself in agreement with much of the content of Alcock's paper. When assessing the robability of the existence of psi phenomena, one needs to consider the implications of its reality, as Alcock says. Are we really going to accept the proposition that the results of scientific inquiry into psi are a product of the psychic influence of the investigator? One might extend the argument: If psi and the experimenter effect exist, they would be expected to exert their influence on other fields of experimental science. How could any experiment in *any* field ever fail to affirm what was being tested for if wishing made it so? [See also Rosenthal & Rubin: "Interpersonal Expectancy Effects" *BBS* 1(3) 1978.] Perpetual motion would have been proven possible long ago because the psychokinetic abilities of dedicated experimenters would have succeeded where physics failed them. Cancer would certainly not be a problem. For each experimental regimen tested, the patients would be cured even if the particular approach was physically ineffective. The desire of the physicians, and certainly the hopes of the stricken, would psychically effectuate a cure. That the world does not seem to work this way appears to negate the possibility of psi's genuineness.

At the same time, I remain unconvinced by the evidence provided by R & P, and I found some of their arguments disturbing. I was bothered by what amounted to their appeal to pity when they complained about the higher level of empirical support demanded of the proponents of psi and by their assertion that the sort of scrutiny and skepticism directed toward psi researchers is somehow unique in all of science. Let me assure them that in any field of orthodox science, when researchers call for a reassessment of basic assumptions predicated on equivocal data, the reaction is the same – skepticism.

Those few scholars who, for instance, claim evidence for the diffusion of cultural traits such as agriculture and urban civilization from the ancient Old World to the New World are greeted by other scholars in prehistoric archaeology with a level of skepticism comparable to that afforded psi. Yet whereas the outcome of this debate might be of tremendous concern to other archaeologists and historians, the implications of the diffusionary hypothesis are not nearly as broad as those of the psi hypothesis. Psi seems to demand a reassessment of our understanding of human biology, psychology, and even physics. R & P's suggestion that the requirement of "extraordinary evidence" for "extraordinary claims" is not justified is itself unjustifiable given the enormous implications of their claims.

Let me present R & P with a scenario in anthropology. They are probably generally familiar with currently accepted constructs of human evolution, so this is probably a fair exercise. Suppose a handful of scholars in anthropology made the following claim: A remnant population of a species of hominid heretofore presumed extinct has been located in the forested American Northwest. Its existence calls into question accepted constructs of human evolution developed since the third decade of this century.

The only data provided are personal anecdotes of sightings, footprints, and questionable photographs. Although not all of these can be explained away, they are neither individually nor collectively convincing. Expeditions have been launched to collect more definitive evidence of the existence of these creatures, but over many years these have only succeeded in collecting more footprints and more blurry photographs. Apparently, the claim is made, there are very few of these creatures, they are extremely shy, and inexplicably, whether or not you can see the footprints and make out the photographs depends on your attitude. On top of this, it cannot be denied that there are charlatans out there making phony footprints and walking around in gorilla suits.

Remembering that the existence of living specimens of a presumed-extinct human ancestor would topple modern human evolutionary theory, one must ask what level of evidence would R & P demand before they accepted the existence of such a creature – more footprints, additional photographs, a live specimen of *Homo habilis,* for example? I would hope the last. Until the proponents of psi can produce the equivalent, they should not expect skeptics to take their claims with less than a grain of salt.

Factual impossibility and concomitant variations

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1. Rao & Palmer (R & P) refer to, and dismiss, Hume's contention that it must not be possible, based on purely historical evidence, to know that a miracle has occurred; or, at any rate, not – as Hume himself adds – if the occurrence of that miracle is to serve as "the foundation of a system of religion." Certainly, as I myself have argued more than once elsewhere (Flew 1961; 1986), this contention, as actually presented by Hume, collapses into incoherence, for its crucial distinction – between the immensely improbable and the genuinely miraculous – is one for which he can make no provision within his system. Indeed, he is actually committed to denying the legitimacy of the notions of factual (as opposed to logical) necessity and of factual (as opposed to logical) impossibility.

Nevertheless, although that is the heart of the matter, it should not be the end of the affair, for it is possible, from suggestions originally made by Hume, to develop a compelling argument sustaining Alcock's conclusion that if science is to be asked to accept the reality of psi, we have to have "a clear, replicable demonstration of a basic phenomenon." Furthermore, this has to be something that there is no question we neither have had nor do have: "a psi demonstration that is replicable in the strong sense (Beloff 1973; 1984; Palmer 1985b)."

The nerve of this neo-Humian argument is that critical historians, concerned to discover as best they may what actually happened, have to identify and assess the available evidence on the basis of their own best-evidenced beliefs about what is (and was) probable and improbable, possible and impossible. From this it follows that all critical historians are, by their cloth, precluded from accepting that something that, as far as they know, is in fact impossible (i.e., could not have happened) did actually happen.

Of course, like the rest of us, they are not infallible. In truth, like the rest of us, they have sometimes been wrong in even their best-evidenced beliefs about what is (and was) probable and improbable, possible and impossible. But, until and unless some particular well-evidenced belief about a factual impossibility has been shown to have been mistaken, the following remains supremely rational both for them and for the rest of us: to go on holding that particular belief to be true and to go on rejecting accounts of how, on some particular past occasion, that belief was shown to be false.

From this perspective, it becomes obvious that the sovereign corrective for erroneous beliefs about impossibilities must be demonstrations that are, "in the strong sense," repeatable: "If it happens," as Aristotle once remarked, "it must be possible."

2. It is not necessary to introduce here the ideologically loaded concept of the miraculous. Both R & P and Alcock agree that the field of parapsychology has to be defined in terms of actual or apparent incompatibility with something very much like C. D. Broad's "basic limiting principles" (BLPs). These BLPs, as R & P point out, "are not the same as the laws of nature, but rather a more fundamental set of assumptions that 'we unhesitatingly take for granted as the framework within which all our practical activities and our scientific theories are confined' (Broad 1953, p. 7)." Two other points need to be made about such BLPs.

First, BLPs are not principles supposedly known a priori, but assumptions sustained by enormous amounts of empirical evidence. So the neo-Humian argument, whcih assumes the factual impossibility of psi phenomena, is not open to the objection made by R & P against Hume himself: that his "maxim is a metaphysical statement, and it is inappropriate to use it when one speaks of empirical evidence."

Second, Broad's own statements about these BLPs always assume a dualist, Platonic–Cartesian view of the nature of man, thus supporting Alcock's suggestion "that the concept of paranormality implicitly involves *mind–body dualism.*" Although Alcock is certainly right in his suspicions that, from the beginning, all or almost all the leading figures in parapsychology have been guided, or misguided, by Platonic–Cartesian assumptions, surely there is no necessary conceptual connection here. Certainly Broad's BLPs can be restated, and all the phenomena or alleged phomena can be redescribed, without any Platonic–Cartesian prejudice. (For further evidence confirming Alcock's suspicions and for specimen restatements or redescriptions, compare Flew 1987.)

3. Alcock complains about "the apparent total lack of constraints on the conditions under which psi can be manifested (whether forward in time, backward in time, across thousands of miles, between humans and objects, between humans and animals, or even between animals and objects." He notes that what is typically done is to "examine two sets of numbers . . . for evidence of a nonchance association."

Much more can and should be made of these undisputatious, yet highly significant, observations. For what, after all, does statistical significance signify? The subsistence of a statistically significant correlation certainly does not *entail* the presence of some causal connection. At most, it suggests that it is well worth looking for such a connection. Accepting that suggestion, the parapsychologists have looked. But – so far at any rate – all they have found are – as Alcock argues – those very statistical anomalies from which all their enquiries began.

What Alcock does not add is how rarely the parapsxchologists seem to have tested for causal connections in the ways that, in practical life, all of us do test. In the whole history of the subject, there has been precious little evidence of what J. S. Mill picked out as concomitant variation. The most striking counterexample that leaps at once to my mind comes from S. G. Soal's since totally discredited work on Basil Shackleton, a work by which so many of us were deceived (Flew 1953). Shackleton scored significantly only under telepathic conditions; his scores dropped to a chance level whenever and for as long as the conditions were, with or without his knowledge, changed to clairvoyant. This surely must be the Royal Road to settling whether or not her looking at the cards is at least a partial cause of his scoring: to remove or at least in some way to modify the putative cause, and then to see whether there are any concomitant variations in the putative effect.

The oddest example of the failure to appreciate this point about causal enquiry is provided by what J. B. Rhine called "the dice work." If you are looking for PK – which, *significantly,* spells out as *psycho*kinesis – then, at any rate, your first tests ought to have been to determine whether anyone could move some very light, very carefully shielded object at will and without either directly or indirectly touching it. Suppose it emerged that the PK effect manifests itself, if at all, only with *collections* of dice or with *collections* of other small objects. Then still, in order to settle whether the subjects' "willing" is having an effect, the subjects should sometimes have been instructed to "will" as the dice were thrown, and sometimes not.

The most damaging failure to appreciate the essential nature of causality has, however, been the admission of precognitive ESP and retrospective PK as possible sorts of psi. For, if and insofar as both are to be defined in terms of "backwards causation," this admission becomes the admission of a conceptual incoherence. Any "backwards causation" would have to involve either making something to have happened that in fact did not happen, or making something not have happened that did in fact happen. But, if this is not self-contradictory and incoherent, then nothing is. So in these cases, although anomalous and statistically significant correlations may indeed have been found, these correlations most categorically cannot point to causal connections.

Evidence of the paranormal: A skeptic's reactions

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To make their strongest case for the "conclusiveness" of psi research, Rao & Palmer (R & P) have cited Helmut Schmidt's (1969a; 1969b) papers. These papers are indeed impressive, but when the authors write that Schmidt's experiments "have been subjected to detailed scrutiny by critics," they mean only that the published reports have been so scrutinized.

Schmidt is a parapsychologist who likes to work slowly and alone. As the history of the field has so dramatically shown, it is difficult for skeptics to evaluate extraordinary results unless they are allowed to see raw data or be present as observers. (The latter is commonly ruled out on the grounds that skepetical mind-sets inhibit psi.) One has only to think of the decades that elapsed before S. G. Soal's critics were proved right by Betty Markwick's discovery (Markwick 1978) that Soal had falsified data (Soal claimed that his raw data had been lost on a train trip); or the years that went by before damaging details were disclosed about the work of Harold Puthoff and Russell Targ, especially about their highly touted die test with Uri Geller (Gardner 1982).

Let me make the following modest proposal. Ray Hyman is widely respected by parapsychologists for his knowledge, fairness, and integrity. If Schmidt desires genuine "scrutiny," let him allow Hyman access to the raw data of his 1969 experiments. If Hyman finds evidence of unconscious bias, Schmidt should welcome the disclosure. What could he lose? I need not remind R & P that withholding, disposing of, or losing raw data, and excluding skeptics as observers, are among the ways that psi research tends to differ from research in other fields.

R & P claim that psi effects are enhanced when efforts are made to reduce noise. I know of no meta-analysis to support this conjecture. Some of the most impressive recent results have been those of Charles Tart, who claims that his hits in one series of tests are above chance, with odds of "a million billion billions to one" (Tart 1976). There is no hint that his subjects meditated or that they were asleep, hypnotized, or sensorily deprived. Hundreds of other tests of incredibly high scores, made without noise-reduction techniques, have been reported. They range from such uncontrolled tests as when Hubert Pearce, in considerable agitation, called 25 correct ESP cards in a row (Rhine, J. B. 1937) to many recent tests for which strict controls have been claimed. I would be surprised if any objective survey showed a significant increase in scoring as a result of planned noise reduction.

R & P also make much of amplification techniques. They single out a card experiment by Milan Ryzl (1966), in Prague, with his star subject Pavel Stepanek. Trained under hypnosis, Stepanek has a longer record of successful card guessing (10 years) than any other subject, though for reasons that remain obscure, he was successful on only one kind of test – guessing which side of a two-colored card was uppermost in an opaque envelope. "Pavel Stepanek's achievement," declared J. G. Pratt, who tested him many times at Prague and at the University of Virginia, "is one that has rarely, if ever, been equaled in the history of parapsychology" (Blom & Pratt 1968, p. 41).

No test could be simpler. If I calculate it correctly, Stepanek's success in the amplified test cited by R & P had a probability of $1/2^{50}$ of having occurred by chance. If Stepanek did not cheat, his success is comparable to tossing 50 coins in the air and having them all fall heads. Of course this probability is much lower than the 1/298,023,223,876,953,125 that Rhine proudly gives for Pearce's 15-card run, but impressive enough to call for hundreds of replications. As is so often the case, some replications with Stepanek by others were successful, whereas others were failures. When Ian Stevenson got chance results in one test, Ryzl (1976) attributed the failure to Stepanek's lack of incentive because of a "reason too private to be discussed here" (p. 91). When John Beloff brought his own cards and rigid covers to Prague for a test, the results were negative (Ryzl & Beloff 1965), much to Ryzl's surprise. And there were many other failures, both published and unpublished.

If Ryzl's tests were adequately guarded against fraud and/or experimenter bias, they should have been a pivotal point in the hoped-for psi revolution. Ryzl now lives in San Jose, California, where he has stopped experimenting to concentrate on teaching, lecturing, and selling his privately published books and ESP training courses. To my knowledge, no skeptic has seen any raw data on his tests with Stepanek, assuming they were preserved. According to Pratt (1973, p. 64), the only records kept by Ryzl were the numbers of correct responses to each run.

Stepanek lives in Prague. According to Ryzl (private communication), Stepanek lost his ESP powers because of emotional conflict over Pratt's work with him. Believers may be puzzled by this well-known tendency of psychic superstars to lose their abilities, but it is no mystery to skeptics. After initial success under loose controls, the controls are tightened by replicators and the ability evaporates.

Ryzl's amplification technique involved repeated guesses by one person. A more plausible way to amplify is to let a group of people simultaneously invoke psi. In the page proofs of *Mind reach*, Puthoff and Targ (1976) described what they called a "proven" system for consistently winning at roulette by group amplification of precognition. (This section was omitted from the book; see Gardner 1981.) Of course nothing could be easier to validate than a proven roulette system.

The notion that a group can amplify PK (psychokinesis) by simultaneous effort is surely promising. Parapsychologists have repeatedly been urged to make and report on the following absurdly simple amplification experiment: Allow a large number of top psychics to gather around a bell jar from which air has been removed. Inside, suspended frictionless in a magnetic field, is a tiny arrow. The PK force needed to make the arrow turn is much less than required to influence a falling die. If PK is a genuine force and its amplification is possible, why does the arrow not move when the psychics will it to do so? Why do all seemingly successful tests of PK avoid direct action to rely solely on subtle statistical deviations, often requiring thousands of trials?

I hope Rao & Palmer agree not to take seriously such reported PK wonders as spoon bending, levitations, finger-ring materializations, thoughtography, and sliding pill bottles. I would be pleased to learn how they justify the repeated failures, over almost a century, of the arrow-moving test.

The case of the underdetermined theory

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On the one side stand those who believe there is scientific evidence that people are able "to receive information shielded from the senses (ESP) and to influence systems outside the sphere of motor activity (PK)" (Rao & Palmer, R & P). On the other side stands one who holds that "parapsychologists have clearly failed to produce a single reliable demonstration of 'paranormal,' or 'psi,' phenomena" (Alcock). On the surface, the debate seems to be between two camps of investigators who differ over the existence of a phenomenon and the scientific status of the data that support it. Were this the case, the advocates of "psi" phenomena would win the day if they were able to deliver rigorous and reliable data in support of their position. And, short of a fully established case (only an ideal within the sciences), a fine-toothed combing of the existing evidence – vast and many-faceted – should finally produce a rapprochement between the two sides. Is there not broad agreement that the validity of a given theory is determined by the weight of the evidence both for and against? Psi advocates R & P do attempt to rest their case on scientific grounds. They also suspect that something more important than empirical evidence may be at stake in evaluating their claims, but they shun this possibility as almost unthinkable in scientific circles:

> If the subjective probability of a disputed claim is zero, then no amount of empirical evidence will be sufficient to establish that claim. In serious scientific discourse, however, few would be expected to take a zero-probability stance because such a stance could be seen to be sheer dogmatism and the very antithesis of the basic assumption of science's open-endedness. (sect. 3, para. 1)

My reading of Alcock suggests that he does give "psi" phenomenon a zero-probability estimate, and therefore he might be considered neither open-minded nor undogmatic when it comes to evaluating the research behind the "psi" phenomena. His review of the evidence is consistently negative; the opposition arguments are never allowed a single score. At the same time, however, R & P seem equally disposed to squeeze an elixir of optimism from the same vineyard. Threats of validity are either dismantled, doubted, or denied. In effect, the outcome of their analysis seems as predetermined as that of the opposition.

Psychologists have generally held to the view that "scientific truths" should rest on empirical foundations. Theories should be data-driven. Yet, based on the work of Kuhn (1970), Quine (1960), Toulmin (1961), and others, it has become apparent to many that scientific theories are notoriously underdetermined by data. Observation is informed by a forestructure of theory, value, and epistomology; it is this forestructure of understanding that is responsible for producing what we count as fact, and not vice versa. This seems highly relevant to the present dilemma. For both the advocates and enemies of psi, it is the forestructure of understanding that seems to determine what the investigators make of the evidence. And we might well expect that no future set of findings will resolve the differences either. Additional research will only generate a further expansion or elaboration of the competing forestructures.

Alcock does seem to perceive this kind of predetermination as being inevitable. He (I think rightly) suggests that one of the major debates between traditional scientists and parapsychologists is about "whether or not dualism, as opposed to materialistic monism, is the correct view of nature and of mankind's place in nature." Alcock believes that the two sides can never meet if the advocates of psi are actually looking to justify "some form of secularized soul." By implication, then, his own analysis might be faulted if one were unwilling to accept an ontology of materialist determinism.

R & P are reluctant to accept a conception of psi phenomena as soul-like. They emphasize that although certain "exchanges of information between living organisms and their environment . . . appear to exceed somehow the capacities of the sensory and motor systems," this occurs because of the ways that these systems are presently understood. In general, R & P seem to side with those who would see the anomalous nature of "psi" phenomena as the result of the faulty limitations on the assumptions of normal science, and not the mistaken perception of phenomena by the parapsychology researchers. Alcock's point nevertheless remains salient, however, as it is difficult to imagine what form of ontology R & P have in mind when they speak of psi events as "shielded from [the] senses" and "outside the sphere of . . . motor activity," unless they have some form of mentalism "in mind." At the same time, if Alcock is to remain consistent, he should be equally opposed to psychologists utilizing concepts of cognition, attitudes, moral judgments, intellectual capacity, and the like. They too are relying upon concepts ungrounded by physical operations, which thus smack heavily of "secularized soul." Does the rhetoric of the cognitivist avoid the pitfalls of the mind–body problem, or is it that positing a rational process is more socially acceptable than positing a soul? I think it is the latter.

If Alcock's criterion were widely deployed, much additional research in psychology would also be impugned. Alcock criticizes parapsychologists for creating explanatory principles that render hypotheses about the "psi" phenomena unfalsifiable. Certainly supporters of "psi" phenomena do tend to defend their work against attacks from critics who fault their findings. They modify explanations to take account of unexpected research outcomes, develop *post hoc* excuses for research failures, and so on. In so doing, they avoid the final blow of falsification. However, such efforts to justify hypotheses are hardly unique to parapsychologists; they appear to be common scientific practice among those supporting one paradigm against another. Despite lip service to Popperian notions of falsification, I think that scientists rarely give up their basic ideas on the basis of unsupportive data. Again, it is not findings but forestructure that counts.

As a general surmise, I think Alcock acts as a gatekeeper for the traditional cadre, trying to keep the riffraff out of the corps. Perhaps it is a necessary rear-guard action that keeps the regulars in orderly form. However, risks to propriety seem everpresent. Graduates of Ivy-league schools major in computer science, and then develop software for reading tarot cards; professors of the natural sciences put aside their research on the weekend to attend religious services; and students of economics, law, and logic snort cocaine, despite medical evidence of the hazards. Astrology, acupuncture, Zen, the Tao, humanistic therapy – these and other mystical attractions find eager audiences among the Western educated elite. Perhaps as long as religion and its secular counterparts create and fill emotional and intellectual needs, the lure of bringing such phenomena as "psi" into the body of science will endure as well. And until such needs and their relationship to the forestructures of observation are examined, debates such as these will remain unresolved.

Axioms in science, classical statistics, and parapsychological research

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Why has the debate about psi produced so little accommodation, so little "progress," so little apparent "resolution"? Perhaps it is because the debaters do not recognize that they are often debating axioms, not theorems. Alcock has suggested a possible axiom dividing parapsychologists and their critics: assumed dualism or assumed monism. Does he believe that empirical data can show which assumption is more questionable? Do Rao & Palmer (R & P) believe that empirical data could confirm any dualism between the physical world and a nonphysical one? Both Alcock and R & P appear united on one matter, however, which I suspect ought to be questioned. I have in mind the universally assumed appropriateness of classical inferential statistics for evaluating the degree of surprise attaching to experimental data when effect sizes are extremely small.

Everyone's axioms deserve explicit attention and listing. Here are seven principles I hold to be true, but *not* self-evident, and certainly they are *not* things that could be proved or disproved empirically:

1. Truly random events are always determined events.
2. Determined events are "truly random" only if, and after, human beings agree that they are.
3. The best statistical definitions of randomness describe the *outcome* of "random generation," *not* the rules of mechanisms that generate that outcome.
4. Some completely predictable events might reasonably be defined as "random" (think of computer-generated pseudorandom digits).
5. Some quite unpredictable events might reasonably be defined as "nonrandom" (think again of those same computer digits).
6. "Unpredictability," if used as a definitional criterion for randomness as applied to outcome strings, is always a statement about the relative degree of the predictor's ignorance and comprehension of what in fact causes or correlates with events. This makes "unpredictability" a statement about the relative state of the predictor's current knowledge and *not* a statement about the mechanism producing the outcome being predicted.
7. Nothing ever *guarantees* that a device that we expect will generate random outcome will in fact do so, especially in the finite short run.

Among the theorems that can be derived from the above axioms is the following (given here without proof – see Gilmore, in preparation): *Every finite symbol string is nonrandom under some nontrivial definitions of randomness*. A very significant corollary is: *Every finite symbol string has nonzero association with the majority of symbol strings of the same type*. One might ask then whether or not the excesses of hits and misses that comprise the experimental evidence for psi actually do constitute surprising and informative departures from the values one would see in a real Monte Carlo world where no psi effects existed, but where ideal randomness did not exist either. It is here, of course, that classical statistical inference is invoked to reassure us.

Classical statistical inference presumes that there is complete independence among every element in our random strings, and this is demonstrable only in the infinitely long run. In the formal world of classical statistics, local departures from independence are invisible because they are camouflaged and swamped by compensating departures elsewhere. They are, in effect, invisible by definition. One might well think that local departures *are* recognized by classical statistics and that they are quantified in the concept of the standard error. But note that standard errors too arise formally from the assumption of complete independence, and they too refer to ideal distributions in the world of the infinitely long run. What is the actual standard error of the standard error? (Classical statistics tells us it is zero, formally.)

The data reviewed by R & P have suggested to them that the psi hypothesis is a compelling one. The same data might also suggest that classical inferential statistics do not provide accurate estimates for areas in the never quite normally distributed tails of the real world. If the real world does *not* match the ideal, then some of the evidence adduced for psi may reflect what I cannot resist calling the "pararandom" nature of Monte Carlo randomizers, that is, real-world randomizers, as contrasted with (unobtainable) ideal randomizers (output from which is free from association with the output of any other selected device).

We have learned that planets do not really move in perfect circles, nor even in perfect ellipses. Why should frequency distributions, in the real world of pararandom critical ratios, follow the perfect normal distribution exactly? It seems to me that *we should all be agnostics, at the least, when asked if we believe that the real world obeys the same laws that apply in the formal world of classical inferential statistics*. I mean no slur on classical statistics, nor upon parapsychologists who are taught a proper reverence for them. But *nothing requires that the real world should behave according to the axiomatic ideal in classical statistics*. Classical statistics have their place. Just maybe, that place is not the real world of very rare events and/or very weak effects.

There are appropriate research designs, and appropriate techniques for statistical inference, when faced with pararandom events. The randomization test (Edgington 1969; Edgington & Strain 1973) is the appropriate statistical technique. Randomization tests are quite unfashionable right now because they seem so crude and so low in power, and because even with personal computers they take a few hours to set up and run. But whatever may be the actual Monte Carlo distribution of rare events in the tails of those statistical distributions of interest to us, randomization tests will not be biased. To use the randomization test well requires the right kind of research design. Here, for example, is how I think Schmidt (1981) should have designed his REG (random-event generator) experiments, and how they should be designed in the future:

Trials should have been of two types: those carried out and those not. Blocks of trials of each type should have been mixed "unpredictably." Trials not carried out should have been 5 to 10 times greater in number than the trials carried out. The mean number of "hits" on trials carried out (the subject uses possible PK [psychokinesis]) should have been compared to the mean number of "hits at random" from the trials not carried out (these are trials run off in microseconds while the subject is doing a distracter task). Then the randomization test should have been used, taking the data from *all* these trials, "randomly" assigning the trial blocks to a new permutation of those to have been "carried out" and those "not," computing the mean-difference score on these new trial types, in this new "experiment," and then a few hundred thousand more such "experiments" should be done, providing us in the end with the Monte Carlo probability for seeing such PK-consistent results in a world where the pararandom target sequences generated for this one experiment (and this subject's particular pattern of "influencing") have been the norm. The data from the original trials, those carried out and those not carried out, should also be (electronically) published so that others may perform their own randomization tests on these data if they wish. Thus, "nonrandomness" in the REG and nonrandomness in the responses of those *carefully selected* human subjects who are used in such experiments would not be able to produce apparent evidence of psi should there be no such thing as psi.

R & P, and others such as Schmidt, may wish to reiterate the claim that the REGs used in published research have been periodically tested and that their randomness has been demonstrated, thus rendering my proposed research design pointless. Such an argument ignores, or denies, the force of the two theorems I proposed earlier. Full randomness in long symbol strings is essentially unprovable (Chaitin 1975), and even modestly random REGs are notoriously difficult to create (cf. Coveyou & MacPherson 1967; Kaplan 1981; Knuth 1969; Marsaglia 1968). Parapsychological research needs protection against the nonrandomness it has yet to notice. The randomization test should not fail to detect any significant effects even if the REGs used *were* somehow to achieve the full randomness that is often claimed for them.

The criticisms of REG experiments implicit in the above apply by extension to many of the other research designs mentioned favorably by R & P. I submit that "randomness" is a far deeper concept than is generally realized, embracing aspects that are not, and cannot be made, reconcilable (Gilmore, in preparation). So much in parapsychology depends so fundamentally on understanding the concept of "randomness," and on understanding the effects of departures from "randomness" that I wonder if it isn't premature to conclude that the data parapsychology offers are anomalous in quite the way they are said to be anomalous. All this is colored by the axioms I listed earlier, however. What axioms could others offer?

ESP and the Big Stuff

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Back in the good old days of spiritualism and psychical research, there were hauntings, levitations, visitations, voices from the dead, protoplasms – something worth getting excited about. Now the parapsychologists can offer us only minute variations in the random flashes of lights, and the assurance that the probability that the variations are due to chance is less than two in one billion. And that is what's wrong with parapsychology. It wouldn't matter if the chances were less than two in one zillion; a few variations in flashing lights are not going to cut it.

The Big Stuff all turned out to be fraudulent. The little stuff – the flashing lights and p values – may be sincere, but it is unavailing, and for good reason. It is not just that the nicely controlled, blind, significantly p-valued experiments are boring (although they are indeed). There's more: By their very design, these experiments cannot establish anything interesting, and parapsychological hypotheses are generally that. The experiments follow a design that is standard in the behavioral sciences; the incompetence of the design is well understood, and frequently noted, although every time a stake is driven through its heart, it manages to climb back out of the coffin.

The statistical design is simple. You take the hypothesis of absolutely no interaction, or absolutely no change in a physical system; you run a large number of trials, and, under a statistical assumption, you find that the outcomes of the trials are extremely improbable given the hypothesis (or, more exactly, the outcomes lie in an extremely improbable region of the assumed distribution). Then you reject the "null hypothesis" that the results are due to chance, and argue that they must instead be due to paranormal abilities.

The p value obtained is not a measure of the "size" of the effect. In general, the effects claimed in these ESP experiments are miniscule. What we can conclude from the experiments is something like this: Minuscule interactions or minuscule changes in a physical system are taking place that are not due just to sampling variations. The trouble is that no sensible person will conclude that extrasensory phenomena are responsible for these changes. Minuscule interactions and minuscule changes in a physical system can be due to an enormous variety of possible, ordinary causes, and we have no inventory of all the experimental protocols and descriptions for some mundane explanation of the minuscule. Sensible people will simply say that as long as the effects are minuscule, there is in all probability some combination of perfectly ordinary causes producing the effects, and no matter what the p value is, and no matter how scrupulous the experimental controls are, there is no reason to take the paranormal seriously. In these cases, explaining the minuscule effect by an extraordinary cause is always cavalier, always unwarranted, and ever unconvincing.

The parapsychologists are not wholly to blame. They have merely adapted to their ends the methods routinely used in psychology and in clinical trials in medicine. The methods are as silly there as in parapsychology, but with this difference: In most behavioral and medical experiments, the hypothesis to be established is either of little interest or is already known to be true. No one worries much about the poor designs because the experimental outcomes do not really matter in these cases. The experiments are more for ritual and full employment than for information.

If the parapsychologists want to convince rational folk, it is easy enough in principle. Use small effects as a trigger for big effects, and thereby amplify the small effects. Then show us somebody who can control the big effects through the small ones. Make things jump about without touching them – big things. Move a mountain or two without applying any force. Let Randi watch. Go for the Big Stuff and everyone will take notice, without the aid of any statistics. Keep the blinds and the controls and all the arrangements designed to prevent fraud or ordinary mechanisms, and rig things so that somebody can make Big Stuff happen. If ESP is the genuine article, it should be possible; until it is done, I think sensible people will rightly be skeptical, and rightly conclude that, even in the absence of specific unparanormal explanations, statistically significant experimental results provide no reason to credit extrasensory mechanisms.

Experimental evidence for paranormal phenomena

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The conventional attitude towards an experiment that is claimed to provide evidence for a new and extraordinary phenomenon is expressed in the following statement:

> In order to assert that a natural phenomenon is experimentally demonstrable we need, not an isolated record, but a reliable method of procedure. . . . We may say that a phenomenon is experimentally demonstrable when we know how to conduct an experiment which will rarely fail to give us a statistically significant result. (Fisher 1942, p. 16)

It has long been assumed that the acquisition of information is mediated by the senses and that repeatable demonstrations to support this assumption are available. The experiments discussed by Rao & Palmer (R & P) certainly fail to do so more often than "rarely." If the methods and procedures in these experiments are reliable, there is no need to invoke a new principle of "statistical replicability" in order to indicate that in certain circumstances sensory information is not required.

It is apparent, however, from the history of research that experiments have been reported using simple methods and procedures that could provide the basis for a repeatable demonstration. Their results indicate that information is not acquired in the absence of sensory information. These experiments have either not been repeated or the methods and procedures have been modified. The effects of these changes are apparent in the ESP experiments discussed by R & P.

Coover's (1917) experiments on telepathy using numbers 1–10 from packs of 40 playing cards produced chance results. From 1928 to 1930, Rhine (1934) also obtained chance results using numbers 0–9. In late 1930, (Rhine 1934) an experiment carried out in conjunction with K. E. Zener, using for the first time the 5-choice ESP cards, also gave chance results.

After Zener dissociated himself from the experiment, leaving Rhine to his own devices, Rhine was successful in providing evidence for telepathy, clarivoyance, precognition, and psychokinesis (PK). Over the following three years, he found remarkable ESP ability in each of six postgraduate students out of the seven who were tested.

Following publication of Rhine's results (Rhine 1934), attempts to repeat his experiments and confirm the findings carried out in five psychology departments failed (Adams 1938; Cox 1936; Crumbaugh 1938; Heinlein & Heinlein 1938; Willoughby 1937). An experiment carried out at London University also failed (Soal 1940). More than 400 subjects were employed altogether in these experiments (Hansel 1961b).

It was pointed out that Rhine's recommended conditions for carrying out experiments were those conducive to experimental error (Kennedy 1939). Rhine himself proposed an "experimenter effect" to explain the inability to confirm his results. [See also Rosenthal & Rubin: "Interpersonal Expectancy Effects" *BBS* 1(3) 1978.]

R & P question whether loose experimental conditions can account for the result of the Pearce–Pratt (Rhine and Pratt 1954) series conducted in Rhine's laboratory. R & P assert that the series was more rigourously controlled than the other early experiments and supported Rhine's point that it did "give highly significant results."

For 35 separate sessions, (Rhine & Pratt 1955) Pearce obtained high scores when situated in a building apart from the targets being handled by Pratt. Records were made by Pearce of his guesses and by Pratt of the targets. These records were to be deposited with Rhine before the two men met after each session. Under these conditions, Pearce, who had previously failed when he was moved more than a yard from the cards, obtained consistently high scores.

After visiting The Parapsychology Laboratory, I pointed out that the experimental conditions were such that it would have been a simple matter for Pearce to gain sight of the targets because he was within walking distance of the experimenter's office and he was left unsupervised during the experiment (Hansel 1961a). I suggested four possible ways in which this might have been done, one of which was to look into Pratt's office through a plate glass window from the corridor (Hansel 1961).

I later pointed out that a full report of the experiment was not published for 20 years. Brief accounts given earlier by Rhine in two journals and in four books contain contradictory statements on the procedure, the recording of the targets, and the scores achieved on different occasions. It also transpired that rather than being the only tests of this nature carried out with Pearce, as was stated in the report, further tests, without success, were made with the subject when he was two miles away and also when he was taken in a car to different locations. After these experiments, Pearce's ESP abilities were said to have deserted him.

The Pratt–Woodruff (1939) experiment placed no. 1 in the 1940 survey is not mentioned by R & P. It is of importance for inicating improved procedures introduced in Rhine's laboratory that were forgotten in later research. Serially numbered record sheets were prepared in advance of the experiment and maintained in an office. It was stated that "three independent records were kept of all score data. Any unwarranted treatment of these data would be impossible without detection by three members of staff of the laboratory" (Pratt et al. 1940, p. 157). The original record sheets for the experiment were kept in the office and were available for inspection after the experiment was completed.

All such experimental safeguards were dropped by Helmut Schmidt in the series of his experiments (Schmidt 1969a; 1969c; 1969b; 1970a; 1970b) chosen by R & P as representing "one of the major experimental paradigms in contemporary parapsychology." Schmidt was the sole experimenter. The only checks on the accuracy of recording and maintenance of records were the nonresetable counters sealed within the machine. Otherwise, the recording of numbers from counters after each test, or the analysis of data from printouts obtained over a period of a week or more would present the same possibilities for error as would counting the numbers of hits and trials on record sheets by a single experimenter.

In only the first of Schmidt's experiments (Schmidt 1969a; 1969c), details of which are given by R & P (sect. 3.1), could these sources of error have been avoided by having the nonreset counters checked by independent observers before and after all subjects had been tested. In all later tests, the two conditions such as aiming to score high or aiming to score low appear to have balanced out so that the overall score as shown on the these counters at the end of the experiment would have been at the chance level.

R & P state that having the two conditions acted as a control against any bias in the random number generator. But Schmidt stated in his report (Schmidt 1969b) that randomness tests were made on a sequence of 5 million generated numbers using the paper-tape printout. These included tests for sequences of numbers that could not have been made using the counters in the machine.

The VERITAC experiments (Smith et al. 1963) referred to were conducted by a team of five investigators in the United States Air Force Laboratories. They used fully automated procedures for generating targets and scoring. After testing 37 subjects using a carefully planned procedure, it was found that neither the group as a whole nor any member of it displayed any evidence for ESP. The safety features of the apparatus were clearly sufficient to remove sensory information. R & P's statement that Schmidt's subjects were "carefully screened through pretesting procedures, whereas those who participated in the VERITAC experiment were not" is misleading. Thirty-seven subjects were tested in the VERITAC experiment. Schmidt chose 3 subjects after carrying out preliminary tests with "approximately 100 persons" (Schmidt 1969b).

The "other examples of replicability" are of a different nature because the method of forced choice from a small number of known simple targets was dropped (R & P, sect. 4.1.2). Targets were pictures such as art reproductions. Complex procedures were used to obtain an assessment of the subject's performance and to control against sensory leakage that might introduce further possibilities for error. This is exemplified by a report (Marks & Kammann 1978) written after the authors had checked the records and procedure used in assessing performance in a remote-viewing experiment.

The "second major research paradigm" (R & P, sect. 4.1.2) can inspite little confidence. My own lengthy appraisal (Hansel 1985) of one of the ganzfeld experiments (Ashton et al. 1981) is ignored. If the remaining 41 experiments mentioned are similar in nature, it is difficult to see why they should be considered for serious attention.

In discussing fraud, R & P question what I wrote about Schmidt's PK experiment (Hansel 1981) and suggest that I provided no evidence that fraud had occurred. It should be clear, however, that I was then replying to a critic who had claimed that Schmidt's work was "the most challenging ever to confront critics and that it made earlier criticisms of parapsychological research obsolete" (reported in Frazier 1979, p. 4). I wished to indicate that the apparatus used in the experiment did not exclude trickery and that it would have been theoretically possible for the experimenter or the subject or some outside person to have brought about the result.

R & P argue that it is impossible to provide a "conclusive experiment" free from possible error or fraud. But the process of repetition itself is sufficient to act as a guard against such factors if it is assumed that not all investigators are incompetent or fraudulent and if skeptics can repeat the experiment for themselves.

The experiments carried out by Schmidt could easily be repeated with changes in the method and procedure to allow for criticism; or further tests could be conducted with the VERITAC procedures. There would appear to be little point repeating the remaining experiments with the present methods of investigation when simple techniques can be used that could save further discussion and provide a repeatable demonstration one way or the other.

"Please wait to be tolerated": Distinguishing fact from fiction on both sides of a scientific controversy

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In order for a scientific controversy to hold out any prospect for an eventual solution, it is one of its minimum requirements that those involved one way or another in the debate try to establish what is fact and what is fiction on either side of that controversy. The authors of both target articles – Alcock and Rao & Palmer (R & P) – have tried to do just that for the parapsychological side in the controversy about the quality and scientific legitimacy and respectability of parapsychological research, and they have reached what appear to be fundamentally different conclusions. I like to think of myself as keeping one foot firmly inside the parapsychological door and the other inside the skeptical, and most people involved in the ongoing controversies over the scientific status of parapsychology seem to consider me either a noncredulous parapsychologist or a moderate critic.[1] It is from this standpoint that I will take a somewhat closer look at the justification for some of the critical arguments that Alcock advances in his article.

Both Alcock's and R & P's articles have their merits. Although the latter is a welcome statement of two leading parapsychologists' views on the state of the art in their field (including research developments and controversial issues within, as well as critical objections to, parapsychology), Alcock's article seems to represent a considerable advance over his book (Alcock 1981) and others of his earlier writings on the subject. Alcock seems to have reconsidered at least some of the opinions he expressed in earlier contributions to the debate. Only one indication of this is the fact that he has now adopted for his own argumentation the distinction between "descriptive" and "explanatory" concepts that has been proposed and discussed repeatedly in the parapsychological literature (e.g., Hövelmann 1983; Hövelmann & Krippner 1986; Palmer 1983) and that Alcock had labeled "nonsense" only a few years ago (Alcock 1983, p. 84). (Regrettably, though, he fails to tell his readers about the origin of this fundamental distinction, and through the use that he makes of it, he even seems to imply that parapsychologists have failed to take it into consideration.)

Justifiable space limitations require me to concentrate on a few particular problems with Alcock's paper, and I will not be able to make more than causal reference to R & P.[2] In many ways, Alcock's article is less important for what it says than for what it fails to say; examples suitable to illustrate this abound. It may therefore be sufficient to mention as briefly as possible (and without discussing any specific details) only a few of them in the following paragraphs (1–3) before I turn (4) to what I consider the main deficiency of his target article.

1. Almost throughout, Alcock fails to provide a reasonably complete and balanced picture of the field he discusses. There are a considerable number of instances where he could have mentioned that many of the criticisms he puts forward are already to be found (and frequently have a long history of extensive and controversial discussion) within the parapsychological literature itself.

2. Upon careful reading, we find that in Section 6 (Are the critics fair?), Alcock is actually addressing two questions that differ considerably from the one posed by the section heading itself. Those questions might be formulated as follows: Are parapsychologists' reactions to criticisms fair? and Why don't critics need to be excessively concerned with being fair? Although Alcock insists that criticisms of parapsychology have been as fair as necessary, there can hardly be any doubt that, almost invariably, the quality of most criticisms leveled against parapsychology in the past has been an embarrassment not only from the point of view of the proponents, but from that of responsible and knowledgeable skeptics as well (Hövelmann [with Truzzi & Hoebens] 1985). Even Hyman, one of Alcock's fellow skeptics and a prominent critic of parapsychology, does not leave the slightest doubt about this fact (Hyman 1985a, pp. 6, 87, 89).

3. Alcock quotes at some length from the seminal "joint communiqué" by Hyman (the skeptic) and Honorton (the parapsychologist) (Hyman & Honorton 1986). However, he omits the three sentences printed right between the two passages he quotes; those sentences would have strongly contradicted the general impression he apparently wishes to convey of the status of parapsychological evidence (see Hyman & Honorton 1986, p. 352, para. 5).

4. Alcock is of course perfectly right that, up to this day, parapsychologists – many, perhaps even most of them – have been guilty of all the sins for which he takes them to task. Nevertheless, there are several nontrivial problems with his argumentation: Alcock's opinion that what parapsychology is actually all about is the "search for the soul" (or maybe even the search for something *extra naturam*) is the unquestioned premise, not the result, of his investigation. This has several serious consequences for the defensibility of his arguments. His premise apparently forces him to present a picture of parapsychology that is both carefully curtailed and distorted in a very specific way, one that cannot in fairness be considered representative of the work and arguments of leading parapsychologists. Alcock correctly distinguishes between the "soul-searching" approach and what might be termed the "anomalistic" approach in parapsychology. He then goes on to contend that, whatever the parapsychologists may be telling us, the former approach underlies just about everything that is being done and said in parapsychology; that there is a hidden agenda behind all parapsychological activities that can be identified as "the quest to establish the reality of a nonmaterial aspect of human existence"; that the anomalistic approach "would no doubt be more acceptable to most scientists," but that it is notoriously underrepresented in parapsychology and "does not really capture the *flavour* of the paranormal" that "finding explanations for ostensible anomalies is not what parapsychology is really about"; that "parapsychology is not primarily motivated to explore anomalies in an open-minded fashion," and so forth.

As I have frequently criticized occult, metaphysical, spiritualist, or exclusively mentalist leanings in parapsychology myself, I think I can state with some claim to credibility that Alcock's description is at best a caricature of the leading conservative, experimentalist circles in parapsychology. Alcock dismisses in a rather cavalier fashion everything that could possibly imperil his claim that parapsychology is essentially and by its very nature a soul-searching endeavor. Of course, Alcock realizes that there are a few parapsychologists who all too obviously fail to fit this picture of the credulous psi-and-soul enthusiasts. He accordingly seems willing to let at least these few parapsychologists off the hook.[3] Those parapsychologists who favor a noncommittal, anomaly-oriented approach are easily and quickly disposed of when Alcock states that he "believe[s] that those in parapsychology who move closer to the skeptical side will fail to draw the rest of parapsychology along with them," and that a "focus on the anomalies" is "unlikely to happen" in parapsychology. However, this sounds more like an empirical question. Alcock's stated, but unsupported beliefs about such current and future scientific–political developments within parapsychology seem to provide his only justification for ignoring the "conservatives" in parapsychology and for concentrating on (and representing as typical) those who may be "searching for the soul."

Moreover, these beliefs of Alcock's seem to be contradicted by the facts. When he asserts that "it is rare to find" the parapsychological literature "treating apparent anomalies in . . . a noncommittal fashion," he ignores considerable parts of that literature, in particular, a position paper issued by the Parapsychological Association (1986) (the only existing professional organization of parapsychologists) in which precisely the noncommittal, anomaly-oriented version of the psi hypothesis that he says is missing is explicitly stated and defended. The definition of "psi" suggested there is as free of the "flavour of the paranormal" as a definition can possibly be. That position paper was written by a committee of the association's members, approved by its governing council, and discussed by its membership at an annual convention. So here we have (in addition to an increasing number of other relevant publications) an easily accessible document that is as official and representative of the parapsychological community's position on this matter as one can possibly hope to find. And yet, Alcock continues to believe that anomaly-oriented approach is minimal in parapsychology. So are parapsychologists indeed driven by the desire to "search for the soul"? Many of them certainly are (and I have almost as little sympathy for their approach as Alcock seems to have); others may be; and still others most definitively are not. All are indiscriminately treated the same way by Alcock.

There is another problem with Alcock's argumentation: Even if his assessment of the motivations behind parapsychological activities were correct, this would be of limited relevance, at best, to the actual research done in the parapsychological laboratory. The results of that research would still need to be evaluated largely independently of whatever pet ideas the experimenting parapsychologist may hold. And how about the laboratory research conducted by those few parapsychologists that meet Alcock's motivational criteria?

Even though this is not always obvious from his article, Alcock is certainly one of the best informed and most rational critics of parapsychology. The field, I am the first to admit, urgently needs relevant, informed, detailed, and constructive criticism, and there is ample room and reason for being skeptical about many (and maybe all) parapsychological claims. Alcock's target article, I am afraid, is unlikely to satisfy that need.

R & P close their target article with the expression of "hope that the climate of scientific opinion will be sufficiently *tolerant* to permit free and open inquiry by those who have the necessary skills and interest" (my emphasis). Alas, even a charitable interpretation of Alcock's way of treating parapsychologists rather reminds me of a cartoon by Unger that I recently chanced upon in the March 31, 1987, issue of the *Los Angeles Times*. Unger's cartoon shows an elderly couple entering a restaurant and facing a sign that reads: "Please wait to be tolerated."

NOTES

1. It should at least be mentioned, parenthetically, that there are good reasons for suspecting that the traditional distinction between *the* parapsychologist and *the* critic is at least a gross oversimplification of the actual state of affairs (Hövelmann 1986).

2. Nor can I discuss here in any detail some other relevant problems such as Alcock's long-discredited opinion (which is apparent from both the abstract and the final sentence of his article) that research cannot be considered scientific unless the reality of its subject matter has been established beforehand. Suffice it to say here that, say, the search for unicorns (or any other chimera the reader may prefer) can of course be conducted in ways that are impeccably scientific.

3. Alcock mentions, for example, Palmer's "conservatism" and "circumspection" and some parapsychologists' reservations about J. B. Rhine's views on the "nonphysical nature" of psi.

Parapsychology: The science of ostensible anomalies

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Alcock seems to differ with Rao & Palmer (R & P) about the subject matter and objectives of parapsychology. R & P assert that anomalies are the subject matter of parapsychology. These anomalies may or may not be paranormal in origin. Alcock argues "that parapsychological inquiry reflects the attempt to establish the reality of a nonmaterial aspect of human existence, rather than a search for explanations for anomalous phenomena." He adds that it is not clear, in this sense, whether parapsychology has a subject matter.

A closer reading of the two positions suggests that this difference may not be as great as it appears. Furthermore, it might be a difference that makes no difference. R & P, it is true, do take the position that parapsychology has a subject matter – anomalies. But they hedge their bets. Perhaps they realize that parapsychology would lose much of its reason for existence if it were simply the study of anomalies, regardless of their origin. So they qualify this otherwise unorthodox definition of their subject matter by talking about anomalies that are "ostensibly paranormal."

Within this context, they put forth what seems like a modest claim when contrasted with the more familiar claims of their fellow parapsychologists. They concede that no current evidence supports paranormal claims. On the other hand, they assert that contemporary parapsychological experiments have established the existence of anomalies.

I will confine this commentary to some implications of R & P's seemingly unorthodox and conciliatory position. I will argue that we should apply the qualifier "ostensibly" to "anomalous phenomena" in addition to "paranormal phenomena." Contrary to what R & P claim, I do not believe that parapsychologists have demonstrated the existence of anomalies. For this reason, I question the additional implication that these anomalies require an explanation. The problem that R & P face, along with their fellow parapsychologists, is the matter of what, if anything, there is to explain.

What do R & P mean by "anomaly"? They inform us that anomalies "seem to involve psychologically meaningful exchanges of information between living organisms and their environment." The experimental evidence for such anomalies consists of statistically "significant" departures of a set of observed numbers from a chance baseline. In an ESP experiment, the numbers are the correct guesses of a target sequence. In a psychokinetic experiment, the numbers are the observed discrepancies between the actual and expected number of binary targets. The generally accepted convention within parapsychology is that the experimenter can claim that the significant departure from the chance baseline is psi (R & P's "anomaly") only if the results have been obtained under conditions that are free from known biases and that have precluded the operation of normal sensory and motor contacts.

In principle, it is impossible to specify in advance every precaution necessary to exclude all normal explanations. In practice, however, parapsychologists and their critics can usually agree on what constitute reasonable safeguards for a particular experiment. Such safeguards would be those that enable us to trust the results of the statistical tests and to be confident that the controls have prevented sensory communication and motor contact. If these safeguards have been inadequate in any given case, the outcome might still have resulted from the operation of an anomalous cause, but we cannot be sure because a variety of other possibilities might have operated.

R & P's claim that parapsychological experiments have yielded anomalies is premature because, as Alcock makes clear, the experiments in question do not conform to accepted standards. R & P use Schmidt's (1970a; 1970b; 1976) REG (random-event generator) experiments, and Jahn's (1982; Nelson et al. 1984) "replications" of them, as their main exhibit. The REG experiments, it is true, provide many attractive features as potential evidence for an anomaly. They also illustrate the serious deficiencies that parapsychologists must overcome before they are ready to put their case before the scientific tribunal.

R & P point to the inadequacies and irrelevancies of the existing criticisms of Schmidt's research. The critics, they say, have not provided plausible alternative explanations of the results. This failure to provide alternative explanations, however, does not justify concluding that Schmidt has demonstrated an anomaly. Schmidt's experiments were inadequate in several ways – many of them documented by Alcock. We do not know whether any of these inadequacies were sufficient to account for the results. The inadequacies were of such a nature, however, that it would be premature to attempt an interpretation until we can be sure that similar results will emerge under conditions that satisfy conventional standards.

Outside critics have not been the only ones to question the methodological quality of the REG experiments. A team of parapsychologists at SRI International found weaknesses in this data base (May et al. 1980). May and his colleagues surveyed all the REG experiments known to them throughout the year 1979 (this included the major portion of Schmidt's experiments) and found that their combined significance was extraordinarily high. They warn us to suspect the observed statistical significance, however, because "all the studies surveyed could be considered incomplete in at least one" of four respects: (1) More than 44% did not report control tests for randomness, and "of those that did, most did not check for temporal stability of the random sources during the course of the experiment." (2) Inadequate information was supplied about the apparatus "to assess the possibility of environmental influences." (3) "The raw data was not saved for later and independent analysis in virtually any of the experiments." (4) "None of the experiments reported controlled and limited access to the experimental apparatus" (p. 8). The same four deficiencies can be charged against the REG experiments that have appeared since 1979.

Similar problems plague other major programs of contemporary parapsychological research. R & P cite my critical survey of the psi ganzfeld experiments. They mention the methodological flaws that I uncovered (Hyman 1985b) and Honorton's reply (1985), which questioned both my assumptions and my method of assigning flaws. They state that "Honorton presented his own analyses, arguing that the replication rate is not significantly influenced by the presence or absence of potential flaws in these studies."

We do not need to go into the details of the debate between Honorton and myself concerning which particular assignments of flaws were justified. Even if we accept Honorton's, rather than my, assignments of flaws to particular studies, just about every experiment in the data base contains at least one of the flaws, such as uncorrected multiple testing, inadequate control for sensory leakage, inferior randomization procedures, incorrect statistical tests, and inadequate documentation. The parapsychologists do not defend these studies on the grounds that they were adequately conducted. According to accepted parapsychological standards, they were not. Rather, they defend them on the same grounds that R & P use to defend Schmidt's experiments. The critics, it is claimed, have not provided plausible mechanisms for how the acknowledged defects could have accounted for the observed results.

R & P do not mention Akers's (1984) survey of the best experiments in contemporary parapsychology. Akers carefully selected the 54 best experiments that had apparently provided significant evidence for psi. Each experiment was then assessed in terms of a fixed set of categories to determine which flaws should be assigned to it.

Akers used a more conservative criterion than did Hyman for assigning a flaw to a study. He assigned a flaw only if the observed deficiency in method was sufficient to account for the observed significant outcome. He found flaws in all but 8 of the 54 experiments. Although the 8 survivors had no glaring flaws, Akers remarked that neither their methods nor their results were notably strong. He concluded that, taken as a totality, this sample of the 54 best parapsychological experiments did not provide evidence sufficient to establish the existence of psi.

R & P question the plausibility of many of the alternative explanations put forth by critics. Some of the criticisms may be implausible and irrelevant, but this is because both the critics and the parapsychologists have prematurely tried to supply explanations where none were called for. If the critics have failed to supply plausible alternatives, however, this does not justify the claim that the experiments under question have provided evidence for anomalies.

The experiments are defective on one or more elementary categories of generally accepted parapsychological criteria. The fulfillment of these elementary standards is important because it is just these standards that permit us to interpret the results of a parapsychological experiment. The claim that a departure from chance is "significant" makes sense only if it is accompanied by reasonable guarantees that trials are independent and that targets have been properly randomized. Such guarantees turn out to be lacking in a large number of the experiments under discussion. Nor does the claim of significance carry much weight if the experimenters misuse statistical tests. Surprisingly, such misapplications of statistical procedures show up in many of the experimental reports. The existence of such flaws, it is true, does not prove that the flaws were responsible for the observed results. Their presence does suggest, however, that the results are uninterpretable. For this reason, I heartily endorse R & P's statement that, "in the final analysis, the case for psi cannot be won or lost by arguments over past experiments, but only by systematic and sustained new research that will survive the test of time." I would add that the parapsychologists should make sure that they conduct this new research so that we can decide whether or not an observed outcome is a significant deviation that we can call an anomaly.

Although R & P's suggestion that parapsychology is the study of anomalies is innovative, I suspect that it merely exchanges one set of complications for another. For one thing, lacking an adequate theory and reasonable specifications, why should we expect that an ostensible anomaly found in one experiment belongs in the same conceptual category as an ostensible anomaly found in another experiment? What is there, other than an alleged departure of the data from a chance baseline, to tell us that whatever was responsible for the difference of 18% from chance expectancy in the psi ganzfeld experiments is the same "thing" that caused the difference of less than 1% from the chance level in the REG experiments? For that matter, why should we assume that the cause of the ostensible discrepancies of around .05% in the Jahn experiments is the same as that which produced the apparent difference of .5% in the Schmidt experiments? No limit exists about the number of reasons an observed distribution might deviate from a theoretical one. To define the subject matter of parapsychology as the study of anomalies, no matter what their source, seems to me to be quixotic, to say the least.

Skepticism and psi: A personal view

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As Alcock says in his target article, "it is easy to ascribe paranormal explanations to odd experiences one cannot readily explain otherwise." One must accept that a certain proportion of phenomena that are regarded by those who experience them as paranormal are in fact not so. But the step involved in going from the fact that some putative paranormal events have normal explanations to the assertion that all such events are of this nature is one that I do not see the skeptics as having properly justified. Although some pieces of everyday evidence for the paranormal (e.g., cases of superficially striking coincidences that can be demonstrated to be of low significance) are indeed of poor quality, others provide evidence that seems to be of an altogether different order of significance. Merely examining those cases that one has reason to suspect of being of low significance proves little. If one is claiming to be operating in the mode of a scientist, one cannot simply dismiss by fiat the mass of data that are apparently very significant.

The standard argument to the effect that we hear only of a selected set of data (because uninteresting events are not reported) must first confront the fact that the proportion of people who report apparently highly significant paranormal experiences is *not* a particularly small percentage of the population. And then, because often the events involved are felt to be extraordinary in some way (and, consequently, small in total number in a given individual's life) *before* there has been any other evidence of a paranormal process having taken place, the idea that the individual simply takes note of the times when rare coincidences happen to occur is not convincing. The intelligent layperson may see a good deal of truth in the kind of argument I have just given, and may accordingly be skeptical in turn of skeptics who pay inadequate attention to them.

A scientist trained to think logically is at risk of being led astray by thinking *too* logically, or by his thoughts being dominated by preconceptions as to what must be the case. Consider, for example, the thinking of Marks and Kammann (1980, Chap. 2) on remote viewing, at the very point where they first began to doubt the reality of the effect (because the "objective" judging did not confirm their subjective impressions). Because the judge had only the remote-viewing transcripts to go on to decide how well a given location matched the results of a remote-viewing session, whereas the subjects had their actual experiences to go on, other things being equal (admittedly they were not), the subjects were better placed than the judges were to comment on the accuracy of a given match. One is not forced by the evidence available to conclude that the subjects did not really "see" the locations in advance; thence that remote-viewing was an illusion, and thence again to construct the somewhat implausible cueing hypothesis to explain the existing situation.

Consider again the supposed healing by the laying on of hands (which may or may not be paranormal to nature). Do patients' conditions really improve or not, and if they do, is such improvement merely the result of chance or is it the result of suggestion? Preconceptions, however strongly held, cannot answer these questions, and I would suggest that those implicit in Alcock's opening paragraph are simply not in accordance with the actual facts.

The analogies made between psi and polywater and between psi and flying cows are misleading, because (to the best of my knowledge) no reputable scientist believes at the present time in either flying cows or polywater. Psi cannot legitimately be put in the category of theories discarded by scientists, and it is inappropriate to suggest that no one should attempt to theorise about psi in terms of quantum mechanics. In other branches of science, theoreticians do not refrain from speculating until the experimental evidence is clear beyond any possible doubt, and I see no reason for so doing in the case of psi.

Wolman's conclusion (1977a) that psi does not appear to overlap with any other disciplines is superseded by modern investigations into quantum mechanics. I quote the summary of an article in *Physics Today* (Mermin 1985): "Einstein maintained that quantum metaphysics entails spooky actions at a distance; experiments have now shown that what bothered Einstein is not a debatable point but the observed behaviour of the real world" (p. 38).

In the case considered in the article, actions cannot be used to transmit information at a distance, but this may well be a consequence of the special features (symmetries) of the vacuum assumed as a background. Research into the Anthropic Principle (Barrow & Tipler 1986) is also indicative, in contrast to orthodox views and independently of psi research, of the possibility of important active connections between the observer and the laws of physics.

Skeptics (e.g., Alcock in his target article; and Marks 1986) seem to see, in the present uncertainty regarding the concept of psi, a reason for characterising parapsychology as a pseudoscience. One can alternatively view the diversity of views as indicative of a healthy situation, albeit of a science that has not yet been established in its final form (despite assertions by skeptics to the contrary, psi research is a progressing field). Consider this question carefully: Does it matter that we are uncertain at the present time what significance to attach to the concept of psi? If the reader's answer is yes, I ask him to examine the history of the concept of heat and then consider the question again.

Never say never again: Rapprochement may be nearer than you think!

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Behavioral and Brain Sciences is to be congratulated for publishing two outstanding position papers on the parapsychology controversy. In these papers, Alcock and Rao & Palmer (R & P) have made admirable contributions to the issues surrounding psi research. A neophyte could have no better introduction to the field than these two articles.

Readers who know little about parapsychology will find that the two contributions survey the history of the field, describe major research efforts, and discuss some of the field's methodological and theoretical problems. Readers will also find it interesting to observe how differently the same topic can be viewed. For example, R & P, in citing Hyman and Honorton's (1986) joint article, stress the authors' conclusion that "there is an overall significant effect in this data base that cannot reasonably be explained by selective reporting or multiple analyses" (p. 351). Alcock, on the other hand, emphasizes that the authors disagree "over the degree to which the current ganzfeld data base contributes evidence for psi" and that they "agree that the final verdict awaits the outcome of future psi ganzfeld experiments" (pp. 352–53). For me, the most significant outcome of that article was the joint collaboration by two adversaries in enumerating recommendations for additional experiments (pp. 355–62).

Another area of controversy in psi research concerns the experimental data collected with Schmidt's (e.g., 1981a) random-event generators. Alcock concludes that these experiments are "plagued by methodological and statistical flaws of one sort or another." R & P, in their description of the same studies, claim that the criticisms, by and large, have not been substantiated.

On the other hand, it is important to observe several areas of congruence between the two papers. Both call for rigorous research, as well as for clarity and precision in presenting the field's concepts. Alcock finds it "commendable" that one researcher (Palmer 1985a) has recommended that the term "paranormal phenomena" be replaced by a noncommittal term such as "ostensible psychic events." Nor do R & P approve of the word "paranormal"; they advise the substitution of a less committed term such as "omegic."

But what Alcock gives with one hand, he takes away with the other, asserting that "it is rare to find parapsychological research . . . treating apparent anomalies in such a noncommittal fashion. Most, in fact, treat psi not as a description of an anomaly but as a causative agent." Alcock ignores the Parapsychological Association's (1985) official statement on this issue:

> Many parapsychologists dislike such terms as "ESP" and "clairvoyance" because they do not constitute an explanation and carry implicit theoretical loadings that may not be justified. A commitment to the study of psi phenomena does not require assuming the reality of "nonordinary" factors or processes. (p. 2)

Alcock goes on to ask whether liaison between psychology and parapsychology is possible and concludes, "I doubt that such a rapprochement will ever occur, for I believe that those in parapsychology who move closer to the skeptical side will fail to draw the rest of parapsychology along with them." Again, Alcock overlooks the Parapsychological Association's (1985) position that

> labelling an event as a psi phenomenon does not constitute an explanation of that event, but only indicates an event for which a scientific explanation needs to be sought. Phenomena occurring under these conditions are said to have occurred under *psi-task conditions*. . . . (p. 2) It is clear that very little is currently known about the operations and limitations of psi phenomena. For this reason, claims of practical application of psi should be treated with extreme caution. Such considerations are especially important when claims are made for medical uses of psi. . . . Skepticism also needs to be exercised in regard to commercial claims that psi ability can be "trained" or used for making personal decisions. (p. 6)

Alcock's complaint can also be answered by the content of an invited article for *Parapsychology Review*, "Charting the future of parapsychology." In it, Hovelmann and Krippner (1986) observe that

> Parapsychology has nothing to lose and much to potentially gain by listening to and collaborating with its critics. . . . We suggest that both parapsychologists and skeptics work hard on eliminating the proponents/skeptics dichotomy. . . . What parapsychology needs in the long run is nonadvocacy as well as active collaboration and common scientific endeavors of all those who prefer to see a solution to the problems of the paranormal, whatever that solution may turn out to be. (p. 3)

Indeed, the Parapsychological Association encourages skeptics and critics to apply for membership; another organization, the Society for Scientific Exploration, is comprised of both proponents and critics.

One of the most important contributions of Alcock's target article is his suggestion that parapsychologists spend more time concerning themselves "with the actual *experience*, or with *how* such experiences are generated, or with *how* the supposed phenomena work." The phenomenology of psi has not been totally neglected, but it has attracted less attention than it deserves (Krippner & Murphy 1973). The few published studies taking this approach have yielded vital information. White's (1964) classic article, summarized by R & P (sect. 5, para. 4), has served as the impetus for several experimental approaches and provides a unique description of what it is like to experience an ostensible psychic event.

Alcock's paper displays admirable restraint, except for those occasions when he makes extreme statements; the use of such words as "never" in a scientific survey are bound to invite trouble! For example, Alcock suggests that "no physical variable has ever been shown to influence the scoring rate in psi experiments." One might find fault with the way the research was conducted, but there have indeed been several recent attempts to relate physical variables to psi. Tart (1986c) attempted to replicate an earlier study by another experimenter that indicated performance on psi tests could be enhanced by having subjects work inside a grounded Faraday cage; an ungrounded, electrically floating cage was used as a control condition. The difference between conditions was significant. Furthermore, there was a significant relationship between high psi scoring and minimal fluctuations in the earth's planetary geomagnetic field (GMF).

Persinger (1986) analyzed the 1886 study (Gurney et al. 1886/1970) discussed by R & P; it yielded 78 cases of ostensible psi events that contained the day, month, and year of the occurrence with no temporal inaccuracies. As a group, these reported experiences occurred on days when the GMF was significantly lower than for the days either before or after the reported experiences. Persinger's work with similar case collections has consistently yielded significant results (e.g., Persinger 1985), and he is also exploring the role of temporal-lobe function in reported psi experiences (e.g., Persinger 1983). These studies represent modest beginnings and need to be replicated by other investigators. But in the meantime, the claim that a physical variable has *never* been associated with psi scoring (or ostensible psi experiences) would be erroneous.

Some psychoanalysts (e.g., Brill 1944) have claimed that scientific curiosity has its origins in sexual curiosity that has been displaced (at worst) or sublimated (at best). Kuhn (1972) suggests that typical scientists design their studies to *support*, rather than to *falsify*, an already established paradigm. In a similar manner, Alcock reads hidden agendas into psi research, claiming that it "is now, as it has been since the . . . beginning . . . , the quest to establish the reality of . . . some form of secularized soul." Aside from illustrative quotations (Alcock 1981), however, Alcock has provided no systematically collected data to buttress this assertion, despite the fact that psychology possesses a number of methods (e.g., content analysis, hermeneutics, psychohistory) that could be used to determine its validity.

Alcock's final claim is that parapsychology "can never become a science" without clear, substantive evidence of a psi phenomenon. This "product-oriented" view of science deviates from the "process-oriented" definition that is becoming increasingly popular. Were the investigators who laid epicycles, phlogiston, and ether to rest "unscientific" because they found no substantive evidence for the subjects of their investigations? It is the disciplined *approach* to inquiry that defines a science, not its ultimate findings. Rosenthal (1986, p. 334) feels that parapsychology is already a behavioral science, and I would agree with him.

When parapsychologists first became organized on a scientific level, they conducted pioneering studies of hypnosis and multiple personalities. The fact that these phenomena are not currently regarded as primarily psi-related does not diminish the contributions that parapsychologists made to rescuing them for science. At the present time, out-of-body experiences, near-death experiences, and lucid dreaming are legitimate topics of scientific inquiry; yet they, too, were originally scorned by orthodox science but taken seriously by parapsychologists.

Parapsychology studies *reports* of ostensible psi occurrence. The majority of Americans now claim to have had this type of experience (Greeley 1987), and parapsychology, at its best, can help to make sense of these reports and, ultimately, to determine "which type of explanation turns out to be most useful" (Blackmore 1983b, p. 142). Parapsychology's subject matter consists of these reports as well as attempts to simulate them in controlled settings. It goes without saying that many of these reports would be expected on the basis of our present knowledge about perception, cognition, and affect. For this reason, it makes sense for proponents and critics to work together; the current rhetoric may divide them, but the subject matter itself should promote unity. If it is ultimately determined that there are ordinary explanations for these reports, parapsychology will still have served an historic role. If, on the other hand, novel mechanisms are discovered for some of these events, science will have been enhanced by psi research.

Parapsychology's critics: A link with the past?

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There is no doubt that the record outlined by Rao & Palmer (R & P), of modest, solid achievement in research, would be enough to ensure the scientific credibility of any field other than parapsychology. Their language is cautious, their claims are moderate, and their evidence seems impressive. It is Alcock who makes the strong and less than fully substantiated claims, insisting that parapsychology is not genuinely scientific, has no really convincing evidence, and is at heart dedicated to the antiscientific demonstration of the reality of the soul. Extreme claims of this sort are characteristic of many critical writings on parapsychology, including those of Alcock (1981), Hansel (1980), Marks and Kamman (1980), and others. Perhaps, in the long-running debate over the legitimacy of parapsychology, it is the behaviour of the critics that needs to be examined and explained more than that of the parapsychologists. Or is there something truly peculiar about parapsychology that makes it attract such criticism?

In a way, it is indeed parapsychology that is strange, not the critics. Alcock comes close to this strangeness when he claims that "the concept of paranormality implicitly involves mind–body dualism, the idea . . . that the mind, or part of it, is nonphysical in nature." However, dualism is not quite the right idea here; Popper and Eccles (1977) argue for a position that *includes dualism without* overlapping the concerns of parapsychologists, whereas Descartes' original formulation of mind–body dualism was expressly designed to safeguard the future of physics (see standard analyses of the metaphysics of the scientific revolution, e.g. Burtt 1932; Koyré 1968). Instead, parapsychology was founded on opposition to the whole conception of science that sprang from the seventeenth-century scientific revolution, a conception that without denying the reality of the mind or soul made them increasingly irrelevant to the real world of physics. The elements in this conception that were most objectionable were the distinction between subject and object and that between primary and secondary qualities. Between them, these elements were involved in a progressive exclusion of human subjective awareness from what was taken to be the real world. This sense of alienation of human experience from the world of scientific fact gave rise to the nineteenth-century romantic movement in philosophy and art, and somewhat separately it gave rise to psychical research and parapsychology. (The source of parapsychology in the underpinnings of the scientific revolution is discussed at some length by Mackenzie and Mackenzie, 1980, and is concisely described by Edge et al. 1986, chap. 12.)

Parapsychology was thus in a fairly profound sense antiscientific at the outset. It was a reaction not only to the reductionist implications of modern science, but also to its presuppositions. This antagonistic relationship was well understood by Sidgwick, Myers, and other founders of the Society for Psychical Research (Gauld 1968), and by Rhine and some other founders of modern experimental parapsychology (e.g., Rhine, J. B. 1955). It was also well understood by their critics, who stressed the scientific unacceptability of the research enterprise, the a priori impossibility of the phenomena, the likelihood of fraud or experimenter error as the explanation for all the significant findings, and so forth (for fairly typical examples, see Munsterberg 1899; Price 1955; for a survey, see Prince 1930).

But what of the present? Many modern parapsychologists have little interest in an ongoing confrontation either with nineteenth-century conceptions of mechanism or with their seventeenth-century underpinnings. This fact has been difficult for many nonparapsychologists, including myself, to understand. Surely the antireductionist basis for the discipline must still be central to it; otherwise, what is there to hold it all together? The sad fact, however, seems to be that many parapsychologists have no more interest in the metaphysical background of their discipline than psychologists have in theirs. The field has become intellectually self-sustaining, nursing its own development with problems, advances, and puzzles, all generated from within the field, rather than through responding to an ongoing external stimulus. As R & P note, in recent years the emphasis in parapsychological research has shifted away from producing more and better phenomena to convince the skeptic, and toward process research, testing models of how psi works. In this respect, parapsychology is settling down like any other scientific specialty.

If many parapsychologists have become liberated from the metaphysical obsessions of their past, however, their critics, at least those who choose to take on the discipline as a whole, have not. Many of the modern critics of parapsychology seem to be just as mindful of the affront that parapsychology offers to the modern scientific worldview as Hugo Munsterberg was in 1899. Their concern is understandable. Parapsychology has never repudiated its antagonistic relationship with modern science; some parapsychologists still affirm it; and the critics themselves have never had the opportunity that the parapsychologists have had of tacitly deemphasizing that antagonistic relationship in order to concentrate on the internal development of the field. If parapsychology continues to grow as a viable scientific specialty, some of its critics may find themselves the last exponents of an otherwise vanished intellectual tradition, a tradition of active *antagonism and mutual incomprehension between scientists of* the normal and those of the paranormal.

Hypnosis, psi, and the psychology of anomalous experience

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The Rao & Palmer (R & P) hypothesis that "a reduction of ongoing sensorimotor activity may facilitate ESP detection" (Abstract) is based in part on the purported enhancement of ESP performance through the use of special techniques such as hypnosis, and in part on the subjective experiences of "successful psi subjects." *BBS* readers should understand, however, that there is *no* acceptable scientific evidence for the hypnotic facilitation of ostensibly paranormal abilities.

R & P cite Schechter's (1984) review of 25 hypnosis–ESP experiments as evidence for the hypnotic enhancement of ESP. This type of claim is not new; there have been similar enthusiastic claims for hypnosis since at least the eighteenth century (Laurence & Perry, in press). From the time of the early mesmerists, hypnotized individuals have been said to possess clairvoyant powers, to be able to diagnose illness by "seeing" internal organs, to read with their eyes closed, to read the thoughts of others, to see into the future, and to age regress to time of birth and even to supposed past lives (for reviews, see Ellenberger 1970; Hull 1933; Perry et al. 1986). We now know that these apparently supranormal effects of hypnotic procedures are due to a complex interaction among generalized cultural beliefs, specific contextual cues, individual differences within the subject population, and the motivation of both subject and hypnotist. Nevertheless, the belief that hypnotized individuals can transcend normal capacities continues to persist.

What is new in Schecter's (1984) approach is the statistical evidence he marshalls to support the replicability of apparently hypnotically elicited extrasensory perception. His vigorous attempt to rescue a methodologically deficient literature, however, is fundamentally misguided. In essence, Schecter's argument relies on the *lack* of a relation between design flaws evident from published studies and the presence of either significant ESP results or nonsignificant results in the predicted direction. As Diaconis (1978) has forcefully argued, however, no amount of statistical analysis will save a poorly designed study or, indeed, a series of studies. Diaconis, a professional magician and statistician, argues further that his personal observations of more than a dozen paranormal experiments revealed design flaws that were not apparent from published reports, an observation that buttresses the inadequacy of Schecter's approach. More to the point, one crucial design flaw is more than sufficient to contaminate a study, a lesson learned from more conventional psychological experiments.

True (1949), for example, found that hypnotized subjects regressed to the day of their tenth, seventh, and fourth birthdays accurately identified the day on which it fell 92%, 84%, and 62% of the time, respectively. Numerous failures to replicate the findings were puzzling, until it was discovered that part of True's procedure was omitted from the final report in *Science* without his prior consent (Orne 1982; Perry et al. 1986). Rather than ask subjects what day it was, the experimenter, who had a perpetual calendar before him, asked, "Is it Monday? Is it Tuesday?" and so on. It became apparent that subtle cues from the experimenter ("sensory leakage" in Schecter's terms) were causing the effect, and not hypnosis at all. In this instance, the offending flaw was discovered; it is no easy task to find the culprit in every instance. It should be noted that a number of replications of the effect could conceivably have been obtained had the methodological flaw remained intact in subsequent studies. We concur with Alcock that better studies are the answer to critics of psi experiments, not attempts to dismiss design inadequacies. Because Schecter's (1984) review also highlighted methodological deficiencies other than the possibility of subtle cueing (e.g., failure to assess individual differences in hypnotizability, questionable randomness of target stimuli, and failure to counterbalance hypnosis and control conditions), the hypnosis–ESP literature offers no persuasive evidence either for the reality of psi or for its hypnotic facilitation.

At the same time, we wish to stress that the failure to support the ontological status of parapsychological phenomena does not undermine the subjective reality of the psi experience. We agree with Alcock and others (Reed 1972; Zusne & Jones 1982) that the study of anomalous experience can profitably be approached from a psychological perspective using the paradigms of normal science. Although mainstream psychology has been slow to examine these experiences, a number of observations appear to warrant more vigorous interest. As noted in both target articles, personal reports of ostensibly paranormal phenomena are widespread, and quasidelusional beliefs in their objective reality are even more prevalent (see Kihlstrom & Hoyt, in press).

Experimental evidence suggests that beliefs concerning personal efficacy with at least some of these phenomena (e.g., telepathy and psychokinesis) can be shaped by situational variables such as choice of target stimuli, prior discussion between "sender" and "receiver," and instructional set (Ayeroff & Abelson 1976; Benassi et al. 1979), although it is not clear whether these types of manipulations engender transient or more permanent beliefs. There is evidence that already existent beliefs in paranormal and related phenomena are extremely resistant to long-term change, even in the face of contradictory scientific evidence (Gray 1985). The relative stability of these beliefs suggests that they may be related to past and ongoing *experience* and to relatively stable attributes of the individual (Nadon et al., in press). Support for this view has come from the study of individual differences in hypnotic talent.

Before discussing the pertinent findings, it is necessary to touch briefly on some current theories of hypnosis. Foremost, hypnosis is a social interaction in which a person experiences anomalies of perception, memory, and action that have been suggested by the hypnotist (Kihlstrom & Hoyt, in press). The ability to experience these anomalies has been shown to be a relatively stable characteristic of the individual (Hilgard, E. R. 1965; Perry 1977); measures of hypnotic talent appear to index the degree to which a person can set aside critical judgment (without relinquishing it completely), and indulge in the make-believe and fantasy conveyed by hypnotic suggestions (Hilgard, E. R. 1977). What unites the various phenomena of hypnosis is that all involve compelling subjective experiences that do not correspond to objective reality; this is particularly so for individuals who fall in the upper range of hypnotic resonsiveness as assessed by standardized measures. Orne (1959) has argued that the "essence" of hypnosis lies partially in the hypnotizable subject's tolerance for this logical incongruity. To this extent, hypnosis has been conceptualized variously as *believed-in imaginings* (Sarbin & Coe 1972), *involvement in suggestion-related imaginings* (Barber et al. 1974), and *imaginative involvement* (Hilgard, J. R. 1979). A similar notion was proposed by Sutcliffe (1961), who characterized the hypnotizable person as deluded in a descriptive, nonpejorative sense, and viewed the hypnotic situation as providing a context in which subjects who are skilled at make-believe and fantasy are given the opportunity to become engaged in both what they enjoy doing and what they are able to do especially well (see also Kihlstrom 1985; Sheehan & McConkey 1982; Sheehan & Perry 1976).

It has been known at least since Faria (1819) that various abilities associated with hypnotic responsiveness are available to individuals in everyday contexts and that these may manifest themselves in a variety of ways. One of the ways appears to involve subjective experiences that are often thought to be paranormal. The capacity to experience hypnotic suggestions, for example, has been found to correlate with belief in the supernatural (Diamond & Taft 1975; Nadon et al., in press). Similarly, in an extensive interview study of very highly hypnotizable women (perhaps the top 4%), Wilson and Barber (1982) found that 92% of these excellent hypnotic subjects considered themselves to possess psychic abilities or sensitivity. Although they differed markedly in personality makeup, all shared a nonpathological syndrome the authors labeled "addiction to fantasy." These subjects reported frequently experiencing unusual subjective events such as telepathy, precognition, automatic writing, and seeing spiritual apparitions. It is interesting to note that most of these subjects also reported a distinct tendency to confuse memories of fantasies with memories of real events in a manner predicted by reality-monitoring theory (see, e.g., Johnson & Raye 1981). This pattern sharply contrasted with that of the low and medium hypnotizable control subjects, only 16% of whom reported similar experiences (see also Lynn & Rhue 1986). Although this difference may be inflated by sampling bias, it indicates a connection between responsiveness to hypnosis and a propensity to believe in the reality of imaginative, illusory, and hallucinatory experiences.

We have pursued this line of inquiry by developing a reliable self-report measure of paranormal experiences with more than 1,000 subjects (Nadon et al. 1987). Results with this measure, which was based in part on Palmer's (1979) work, confirmed that many college students report these types of experiences. More significantly, we found that these reports correlated substantially ($r = .51$; $p < .001$) with a measure of "imaginative involvement" in sensory and aesthetic experiences (Tellegen's [1981] "Absorption" Scale). We also found (with a subsample of 219 subjects) that reports of paranormal experiences correlated with hypnotizability ($r = .22$; $p < .01$) as assessed by the Harvard Group Scale of Hypnotic Susceptibility, Form A (Shor & Orne 1962) and that they accounted for variance in hypnotizability over that accounted for by Tellegen's measure.

Thus, methodological and statistical tools presently available appear to offer an opportunity to fulfill the promise of William James's interest in parapsychological phenomena. Although he has been castigated for what some have regarded as an overly credulous approach, it was the *experience* that primarily interested James for what it could tell us about the mind. From this perspective, future research needs to elucidate the cognitive nature of anomalous experiences and to explore further the situational and dispositional factors implicated in their occurrence.

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On rustles, wolf interpretations, and other wild speculations

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- Yesterday, while I was walking alone in the forest, I was attacked by 100 wolves.
- How do you know there were 100? Did you have a chance to count?
- Anyhow, there were at least 50.
- Couldn't there have been 20?
- Why haggle? Isn't even one wolf dangerous enough?
- And did you actually see that wolf?
- So, what else could have been rustling there in the bushes?

(A Jewish joke)

Is there a rustle? Let us presume that (a) a given research hypothesis is a priori as likely as the null hypothesis and that (b) no manifest methodological deficiencies are found. What would be the threshold proportion of significant studies for "taking the effect seriously"? Statistical reasoning is not enough, because it assumes independent samples of a known data generator, as well as a perfectly reliable process of handling the data. Even very small percentages of errors, hidden artifacts, dishonesty, or nonindependence, let alone publication bias, would make the proportion higher than that expected by chance. It is thus doubtful that there exist in psychology many null hypotheses that cannot be brought to the point of rejection by some form of statistical meta-analysis. In the absence of a dependable algorithm, it is left to the scientist's discretion to decide in any particular case whether methodological noise is masking a genuine effect or, rather, producing an artifact.

Of course, the discovery of antecedent conditions would help to establish the effect, and it takes some time before these are found. But what if such conditions have not been successfully uncovered in a very long period? Then the existence of an effect that shows up much less often than it fails to show up cannot be claimed without specifically accounting for the alleged failures. Thus, the burden of proof in such a case is *not* on the skeptics. Furthermore, there is more reason for doubt if the proportion of significant results does not increase over years of research. Presumably, in psychology no effect would be considered as even tentatively supported, much less established, by such evidence.

The psi hypothesis seems to be in even worse shape. If the critics are right that many of the psi studies have already been shown to be flawed, rather than just suspected to be so, then the replication-rate figures quoted by Rao & Palmer (R & P) must be grave overestimations.

Does the loudness of the rustle matter? I agree with R & P that for basic science the existence of a psi effect is far more important than its magnitude. However, at this stage the issue is not whether a proven effect is large or small, but rather whether the effect exists at all. For this question, effect size is crucial. It is common wisdom that *any* null hypothesis can be rejected with a large enough N, largely because no amount of experimental control can guarantee that an experiment is not systematically biased. Hence, the magnitude of an observed effect is diagnostic of existence: The smaller the effect, the more likely that it is due to an artifact.

How likely would a rustle be? Skepticism is sometimes mistaken for prejudice. Whereas it would be silly to dismiss plain facts only because they are surprising, it is sheer prudence to judge uncertain evidence view of its compatibility with expectations. An insurance agent who doubts a client's claim that his car collided with a UFO is not being narrow-minded, but is, rather, exercising caution and sober judgment. Science would thus do well to consider the compatibility of a datum with prior *knowledge*. This maxim underlies the Bayesian approach to inference. Although Bayesian statistics in itself is fairly problematic, the main principle of Bayesian inference can hardly be disputed. The reason is that science should be (and, one hopes, is) a flexible but viscous system, one that can move from a local equilibrium state to a globally better one yet remain stable once it reaches an equilibrium. If it were not, we would be continually wavering among alternative hypotheses. Because vacillation is undesirable, but so too is prepossession, the Bayesian approach seems to represent a golden mean. Now, because the prior probability of the psi hypothesis, even merely as an empirical anomaly, is extremely low, applying the Bayesian rule to the data, questionable and undiagnostic as they already are, must be a *coup de grace* to that hypothesis.

R & P invoke the principle of parsimony to rescue the hypothesis. However, the idea that an effect exists is not more parsimonious than the set of sensible artifactual explanations, for the simple reason that the former is not an explanation at all. Strictly speaking, R & P do suggest an explanation for positive results, namely, the omega notion. However, as it stands presently, omega is merely a blanket term. Without further specification of mechanisms or laws, attributing any apparently bizarre effect to omega is much like blaming the Devil for the disasters of 1986 instead of looking into facts and theories in seismology, climatology, nuclear technology, and so forth. Because such an attribution can be applied to any phenomenon, it *explains* nothing.

How plausible is the wolf interpretation? So far, I have only discussed the issue of the empirical likelihood of the effect per se. However, influences are not independent of theories: Even an improbable result would be given more credit if it were predicted from a plausible theory. Conversely, even a seemingly supported effect would be suspect if it implied an absurd theory. Imagine, for example, that 20% of the people who reached the peak of Mount Everest claimed that the snow up there can burn. The other 80% report negative results. What should one conclude?

The omega notion is extremely implausible, not so much because it posits yet unknown mental or physiological entities but because it defies universal laws of physics: To renounce the generality of the inverse-square distance law, we would need much better evidence than for, say, rejecting the hypothesis that there are no photoreceptors on the skin surface. Yet, R & P suggest that on the basis of a small and questionable replication rate, we should go ahead and do just that.

How can we tell a wolf from a nonwolf? The omega hypothesis conflicts not only with the findings of science but also with its *foundations*. For example, it disputes the postulate that causality is unidirectional in time and that there exists a knowable truth that does not depend on the knower.

These keystones of scientific thought are, of course, derived from our general worldview, which could be wrong. It is legitimate to suggest that it might be wrong, but how can one test that? Testing between alternative ideas requires that they lie on some common ground. Alice would have found it frustrating to use science for exploring the world of the looking glass. Yet parapsychology purports to use scientific techniques that are based on some premises (like the axioms of mathematics) to undermine other premises that are not less evident. Hence, by its very nature, parapsychology is not a science; neither is omega a scientific theory or paradigm. Omega seems rather like part of a different meta-paradigm for conceiving of Nature, one that competes with science. Scientists may be converted to it if they ever disappointedly realize that science as a whole is inherently incomplete and internally inconsistent and that omega offers a more adequate solution. Advocates of omega cannot reasonably expect scientists to be convinced by a few anomalous findings, let alone dubious ones.

Furthermore, for a revolutionary form of thinking to win the empirical validation contest, many robust findings that defy customary explanation would have to be collected by scientists within their *own* disciplines. Disciplines are defined by research domain. Wealth of data or specialization of methods may lead to bifurcation. However, an odd hypothesis does not warrant that its proponents form a separate discipline. Accordingly, whether or not anomalous phenomena such as above-chance guess rate really exist is an issue for *psychology*. To be established, if ever, to the satisfaction of psychologists, people who report such effects should publish in the *Journal of Experimental Psychology*, the *Quarterly Journal of Experimental Psychology*, and so forth. Otherwise they cannot expect to do much more than to persuade one another. As an analogy, imagine the attitude of psychologists to the claim of unconscious perception, had its believers formed their own associations and journals.

The risk in crying wolf. Scientific inference should not be biased by possible social consequences, yet scientists' behavior should.

Scientists sometimes jump to conclusions. Fortunately, the only witnesses are usually their colleagues. Even when a false alarm is more widespread, its cost is often limited. For example, if the semantic distance effect (e.g., Collins & Quillian, 1969) proves tomorrow to be artifactual, mankind would not be terribly upset. In contrast, claims about parapsychology are more visible to laymen. Many people cannot critically evaluate the evidence or its implications: Some may interpret as telepathy any case of coincidence of thoughts; some are prone to be overimpressed with sporadic positive results of psi studies often misrepresented by the media; and some may even infer the existence of omega from the fact that parapsychology is defined as an independent scientific discipline on the ground that there cannot be smoke without a fire. A premature conclusion about psi may needlessly shake people's faith in science, if not their entire worldview. Also, the risk of abuse is enormous. Hence responsibility calls – here more than elsewhere – for restraining hasty speculations.

Mind–body and omega. Is there a philosophical doctrine that entails omega? Alcock seems to suggest that mind–body dualism does. I disagree. Believing in a nonmaterial aspect of human existence implies nothing about the causal status of that aspect. A separate psyche may be autonomous (parallelism), affected by the body (epiphenomenalism), or interacting with the body (interactionism). The thesis that a person's mind or spirit can directly interact with the environment, bypassing that person's own body, is an entirely different story.

When immovable objections meet irresistible evidence: A case of selective reporting

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Alcock purportedly presents a conservative view of the evidence for parapsychology. However, he reveals a peculiar bias in his closing arguments – he claims that parapsychology is and has been since the begining a "quest to establish . . . some form of secularized soul." He maintains that because there is no agreement on the subject matter or on the evidence for parapsychology, it is therefore not a science. Surely this is circular reasoning, for science is not only a catalog of knowledge, but also a method by which new knowledge may be sought.

Our reading confirms what Child (1985) and others have found – that some critical reviewers of parapsychological research systematically distort the contents of the literature. Unfortunately, because most academicians are not familiar with the pertinent literature, and therefore cannot detect such distortion, many will assume that Alcock's target article fairly represents the "loyal opposition" on this topic. We believe this would be a serious mistake, and we present three examples of selective reporting to illustrate our point:

1. For his discussion of REG (random-event generator) experiments, Alcock selects a subset of the work of one author (Schmidt) as representative of the entire literature, focuses on a handful of his early experiments, cites oft-repeated criticisms of those papers, and then implies that Schmidt never addressed those criticisms. He does not mention that during the past two decades, literally hundreds of REG experiments have been performed and published by some 30 independent researchers (May et al. 1980; Radin et al. 1985), including experiments that both (a) address all of the criticisms that he and others have raised *and* (b) provide strong evidence for operator-related anomalies. In particular, although he does say that parapsychological research is being performed at prestigious institutions such as Princeton University, he does not describe or address the relevant data (e.g., Jahn 1982; Jhan & Dunne 1986; Nelson et al. 1984; Nelson et al. 1986).

On this same topic, in discussing the quality of parapsychological experiments, Akers's (1984) reference to a paper by May et al. (1980) is cited to support the claim that no methodologically sound PK (psychokinesis) experiments have been performed. However, Alcock does not mention that May et al.'s remarks were part of the introduction to a report describing an REG experiment specifically designed to incorporate all previous criticisms. He therefore did not consider their conclusions, which were: (a) "We have observed an anomalous and, as yet, unexplained effect"; (b) "the magnitude of our results is commensurate with previous reported studies"; and (c) "precautions and controls significantly exceeded any former experiments" (May et al. 1980, p. 35).

2. In his section on remote-viewing studies, Alcock asserts that no studies have been designed with control conditions that allow scoring relative to chance expectation. Again, he overlooks a body of data generated (at Princeton) as part of a program to develop an analytical judging procedure for remote-viewing materials. This computer-based procedure uses scoring algorithms that not only match perceptions against targets but also compare perceptions against the pool of all mismatched targets, thus providing a distribution of chance scores. The data base generated to test this procedure has more than 300 formal remote-viewing trials, and in the aggregate, shows significant

evidence for anomalous acquisition of information (Dunne et al. 1983; Jahn 1982; Jahn & Dunne 1986; Jahn et al. 1980; Nelson et al. 1984; Nelson et al. 1986).

3. A laudable effort to develop a common viewpoint between critic and researcher is discussed in Alcock's section on the ganzfeld literature. He summarizes Hyman and Honorton's (1986) article by juxtaposing two critical quotes, and the impression is given that these two quotes appear as connected paragraphs. He concludes that evidence for the ganzfeld effect must await future research that is more carefully conducted. A more even-handed representation of this paper would have included the paragraph sandwiched between the two quotes, in which Hyman and Honorton state their agreement that the statistical evidence observed in the *existing* ganzfeld data base cannot be explained by flaws such as selective reporting or multiple testing.

In summary, given what is known about the effects of preconceptions in the evaluation and interpretation of empirical data (Diaconis 1985), and the perseverance of beliefs in the face of disconfirming evidence (Ross & Lepper 1980), we would be surprised if Alcock found in the parapsychological literature anything *but* "a hundred years of failure." As Ross and Lepper put it, each of us tries to "maintain coherence and stability in [his] belief system, even at the cost of occasional logical or empirical inconsistencies" (p. 31).

Are the conventional explanations of psi anomalies adequate?

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I agree with Alcock – although not entirely for the same reasons – that parapsychologists have yet to establish that ostensible psi anomalies require omegic (paranormal) explanations. A more timely question to me – and I suspect to many other *BBS* readers – concerns what alternative conclusion should be drawn: Do the ostensible anomalies have adequate conventional explanations, or are they genuine anomalies not yet adequately explained by anyone? Alcock did not directly address this question in his target article, but I gather from his remarks there and elsewhere (e.g., Alcock 1981) that he would say that the alleged anomalies do have adequate conventional explanations. If I am wrong, I trust he will correct me. The issue is important: If psi is a genuine anomaly (as I contend), it is fair to ask that scientists take it seriously and remove the virtual taboo on the subject that presently exists inside academia.

As far as psi experiments are concerned, these alternative explanations are isomorphic with the various "flaws" Alcock discusses. As such, they must be judged by the same standards of plausibility and empirical evidence as any other scientific explanations. However, critics like Alcock rarely apply such tests and seem reluctant to suggest that the flaws actually account for the data. Instead; they argue, as Alcock does (citing Hyman; sect. 3.1, para. 3), that the flaws are "symptomatic of lax research standards." This presumably means that the identified flaws point to more serious (but unidentified) flaws that do account for the data.

Logically, this argument seems to be a non sequitur. We all have experienced how easy it is to make mistakes that, with the benefit of 20/20 hindsight, leave us wondering why we did it that way. In fact, conducting flawless experiments is not nearly so easy as Alcock implies (sect. 3.1, para. 4). A former editor of a major psychology journal observed in an earlier *BBS* commentary that "I have never seen a piece of psychological research that could not be faulted on methodological grounds" (Hogan 1982, p. 216). Care does not guarantee flawlessness, and flaws do not necessarily imply carelessness.

The appearance of psi research as highly flawed is exaggerated by the liberal attitude critics such as Akers (1984) and Hyman (1985b) often adopt in defining what constitutes a flaw. For example, Akers (1984, p. 128) assigned a flaw to an ESP-hypnosis experiment by Casler (1964) in which the experimenter attempted to eliminate unconscious whispering between a sender (a paid experimental assistant) and a college-student subject located in an adjacent room (so the experimenter could observe them both) by playing a phonograph record. Ordinarily, one would consider attention to such a bizarre counterhypothesis to reflect great circumspection. Akers simply labeled Casler's control of such incidental cues "dubious." Later (p. 138), he assigned another flaw because the assistant could theoretically have cheated.

Many of the flaws assigned by Akers and Hyman are not confirmed flaws in the procedures but, rather, failure to document details of procedure that *could* have been conducted in a flawed manner. Such criticisms often assume the very carelessness they are supposed to demonstrate. For example, Akers (1984, pp. 139–40) assigned flaws to 8 of the 54 experiments he surveyed, because the reports did not specify steps taken to assure accuracy of data recording. Details of such routine experimental procedures are rarely included in experimental reports, for economic reasons, and their mere absence provides no basis for speculating that the procedures in question were carelessly conducted.

This is not to suggest that research and reporting procedures in parapsychology (and other sciences) cannot stand improvement. Also, critiques such as those of Akers and Hyman are valuable in alerting us to possible counterexplanations. However, the flaws they cite do not justify the wholesale characterization of psi research as sloppily conducted, as Akers (1985) subsequently acknowledged. Nor do they generally supply plausible or adequate explanations of psi data. For example, it is most unlikely that the slight departures from randomness that *might* be introduced by shuffling methods of randomization (c.f. Hyman 1985b) – assuming they are performed with reasonable competence – would have any practical consequences in an ESP experiment, even though this is theoretically possible. What convinces me that some psi data may ultimately require an omegic explanation is not that I consider any psi experiments beyond reproach, but that the conventional alternatives are, generally speaking, so deficient that even their strongest and most sophisticated proponents are reluctant to acknowledge them as interpretations of the data.

Moreover, it is not flattering to conventional scientific theory to expose how facilely it allows *ad hoc* hypotheses to explain away its anomalies. When astrologers and psychoanalysts do this, it's called pseudoscience; when critics of parapsychology do it, it's called scientific rigor.

I will conclude by commenting briefly on some statements in Section 5 of Alcock's paper that I find incorrect or misleading. A conclusion of psi-missing, just like psi-hitting, requires a statistically significant departure from chance and (assuming use of two-tailed tests, when appropriate) is falsifiable. Many decline effects involve results too strong or persistent prior to onset of the decline to be explicable as regression artifact, and there are no systematic reviews supporting the often-heard contention that declines are correlated with tightening of controls. I question whether scientific conclusions should be drawn from what unnamed parapsychologists may tell critics such as Blackmore at cocktail parties or other such gatherings, and I am not aware of even one example from the modern research literature where a prominent parapsychologist has explained away chance results as a negative experimenter effect. Our own target article clearly illustrates that negative results are accepted as such in literature reviews and meta-analyses. Parapsychologists, like other psychologists, are sometimes guilty of overinterpreting *post hoc* effects, but much of the evidence for psi comes from predicted or cross-validated findings. Although most parapsychologists do conceptualize omegic processes as being space–time independent, psychological constraints are frequently invoked, and these have allowed for hypothesis testing and model building.

Psi in search of consensus

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I can think of no better rapid, in-depth orientation to the state of the art in parapsychology and "antiparapsychology" than the accompanying target articles. This is surely true for both the initiated and the uninitiated reader. Yet the uninitiated reader must despair: How can serious, astute reviewers of a field of empirical research come to such disparate conclusions? Indeed as I understand it, this is in itself the nub of Alcock's argument: Given that the evidence for psi phenomena after 50 years of research is still not compelling and that the controversy persists, then something must be amiss. But need it be so? There are examples even in the physical sciences of issues that are difficult to resolve. The Kennedy murder is a classic example; in their investigation, experts were unable to reach any consensus on the number of assassins involved, despite the "hard" accoustic, ballistic, and photographic evidence. Likewise, in the contemporary case of the murder of Olof Palme, there is disagreement among ballistic experts as to whether the bullets discovered were the murder bullets or were planted ones. (In July 1987 the Swedish police finally announced they were satisfied that these were the murder bullets. They used a statistical method of "blindly" matching the residual particles on Palme's clothes to various bullets – a standard method in psi research.)

To take an area nearer to hand, a recent review (Rose et al. 1984) of research on the heritability of schizophrenia has reawakened the whole debate by casting doubt on the methodological adequacy of the studies concerned. Given the meager resources allocated to parapsychological research and the intrinsic methodological difficulties in this area, maybe one should not be too hard on the parapsychologist in expecting a clear consensus of opinion on the reality or nonreality of psi-phenomena.

In fact, in the present debate there is some agreement about the fundamental difficulty of defining what would be considered an acceptable level of replication and about the fact that no one experiment alone can suffice. There is further agreement that psi experiments are replicable in the general or "weak sense" of the basic findings being confirmed by others, but not in the "strong sense" of being able to tell the critic how he himself should go about getting positive results. Naturally, for Alcock such weak replication as exists can merely reflect replication of errors. In this sense, the theme of such exchanges between critics and proponents of parapsychology is not new; it is only the players who have changed. In the late 1930s, J. L. Kennedy (1939) debated with Gardner Murphy (1938) the possibility of sensory cues and motivational errors as explanations for psi. The American Psychological Association's Review Committee (1939) actually repudiated this as a viable explanation, and it is now generally accepted even among critics that fraud is the only "normal" alternative explanation for the early findings of Rhine et al. (1940) and Pratt, and Woodruff (1939).

What is new is that the field has continued to produce successful subjects and experimenters, albeit in limited numbers. For me this is the crucial point: It is true that it is theoretically possible with some ingenuity to devise normal explanations for virtually all parapsychological findings, but the probability for *all* these contrived explanations being valid is beyond the limits of (my) common sense. We do well to remember William James here, to the effect that only one white crow is needed to disprove the law that all crows are black. Although there may be a dearth of facts about the phenomena under investigation, positive findings have continued to accumulate, and it is this that distinguishes the field from dead ends in science. N-rays, bions, and Martian canals were observed by, at most, a handful of researchers and did not last more than a decade. The alternative explanation that the 50 years of replications (in the weak sense) is merely repetition of methodological errors and fraud – in many cases by reputable scientists – would undermine confidence in behavioral science in general.

Of course there are areas – such as the previously mentioned research into the heritability of schizophrenia – where scrutiny of the evidence reveals gross flaws in most of the studies. However, these areas of behavioral science have never been subject to the continual scrutiny and methodological refinement that have occurred in parapsychology. For this reason, I would place much greater confidence in the consensus of opinion from weak replications in parapsychology – that psi phenomena exist – than in the findings of many areas in general psychology. Indeed, I think the reader will find that most of the specific criticisms of individual experiments that Alcock puts forward are adequately answered in the Rao & Palmer (R & P) review. Where I think Alcock and I might agree is that whatever the nature of these so-called anomalies might be, it is important that priority be given to the resolution of the issue. I am most sympathetic to Alcock's view that it is only replication in the strong sense that will satisfy him. By this he means that parapsychologists have to be able to tell him or other critics how they themselves can get positive results. I think this is an important challenge and one that I would like to try to meet, but before doing so, there are some further points of difference to clarify.

Alcock finds it highly suspect that parapsychology has historically and philosophically been used to support dualism. This is certainly a valid statement, and I think Beloff (1962) is right in saying that parapsychology is the ultimate battle ground on which theories of the mind/body relationship will be fought. But is this a sound basis for discrediting a field of empirical research? Many areas have philosophical assumptions and implications that may prove false or useful. In a similar vein, it is surely irrelevant, at least as regards the validity of the phenomena in question, to speculate on the psychodynamics and cognitive functioning of believers versus skeptics. No one, to my knowledge, has tried (at least in print) to discredit Hans Eysenck's (1986) critique of psychoanalysis (or his belief in behavior therapy) by examining Eysenck's own early psychodynamics or cognitive functioning! Alcock reprimands Tart (1982; 1984) for psychoanalyzing critics, but does not mention that much of his own work as a critic can be described as a cognitive analysis of proponents of parapsychology.

Alcock's general criticism of parapsychology and parapsychologists does encompass two points that I think are constructive and worthy of comment, however. The first is the negative definition of psi phenomena by the exclusion of "normal" explanations. Clearly this is unsatisfactory, and I have often wondered if it would not be better if parapsychologists met Alcock's argument head on and defined parapsychology as the study of phenomena that apparently relate to the nature of the interaction between consciousness and the brain by suggesting that consciousness can directly influence external events.

Naturally, many *BBS* readers will object to such a definition as too all-encompassing and already in part corresponding to the field of neuropsychology. Yet if psi phenomena have the validity claimed for them, then parapsychology must form a confluence with neuropsychology. Others (including, I would think, Alcock) will object that such terms as consciousness smack of dualism. Nevertheless, regardless of the philosophical school we may back, such terms as consciousness, experiencing, cognitive processing, and construing (to name only a few) are now the very substance of a modern psychology where even behavioristic psychology has gained a cognitive slant. In my own field of developmental clinical psychology, one cannot get very far in research without using such terms as attachment, object, object relations, self-concept, and self-esteem; even "dissociated states of consciousness" is now an area of active interest. These concepts, rather than referring to epiphenomena to be eradicated by reductionism, appear themselves to reflect consciousness' deterministic influence on human behavior.

To quote Michael Rutter (1986), a foremost authority in this area (and a psychobiologist of the Adolf Meyer school rather than a phenomenologist): "The notion is that individuals from quite an early age develop affective and cognitive mental representations of themselves and of their attachment figures; these models serve to fashion later relationships" (p. 814). The point made here is that, rather than being a dualist anachronism left over from behaviorism, parapsychology is clearly in tune with the contemporary view that consciousness and experiencing have a steering function in the organism. Acceptance of this does not imply a commitment to dualism, only that there is a philosophical problem that may well be resolved by, rather than created by, parapsychological research. It is possible that our monistic and dualistic philosophies will eventually prove to be too simple.

I am not entirely in agreement with R & P either as regards the legitimate subject matter of parapsychology. They attempt to placate the critic by using the expression "psi anomalies" as a noncommittal term that can conceivably include unknown normal explanations as well as paranormal ones, the latter being labeled by yet another neologism: "omegic." This proposal may appeal to Alcock because he approves of Blackmore's (1983a) suggestion that parapsychology should focus on spontaneous psychic experiences and should seek normal explanations for these in terms of misperceptions and cognitive biases. Parapsychology might thereby gain acceptance as branch of cognitive psychology.

I do not deny that such research just might reveal important factors governing the occurrence and recognition of both paranormal experiences and erroneous observations, but this would amount to a diversion of effort and funding if we are to take the indications of 50 years of research seriously. Linking psi with other "anomalies" as R & P do also has the dire effect of assigning "guilt by association." These other anomalies include all and sundry, and are invariably more poorly attested to and researched. As Alcock observes, parapsychology already has sufficient difficulty in shedding its occult associations. Yet, I see the attempt to find common ground between the critic and parapsychologist a vital step if progress is to be made. Cooperative joint research projects may be a more productive way of achieving this than trying to redefine the field in a way that may blur and compromise the issues.

The remaining general criticism that Alcock directs at parapsychology is its use of *post hoc* explanations ("the experimenter effect") and an attitude he describes as "anything goes" regarding explanations. The criticism may have some justification in the first instance. I myself have written extensively on the so-called experimenter effect and have argued that the use of the concept should be limited to deriving specific predictive hypotheses rather than as a synonym for "failure to replicate." I believe that parapsychology is now in a position to formulate some of these predictive hypotheses.

I am less receptive, however, to the critique "that anything goes in parapsychology." This seems to me to be hearsay when one recalls that researchers are as often as not accused of lacking adventurous and creative thinking in a field where extraordinary phenomena *may* require extraordinary explanations. There can be no fault with creative thinking within the constraints of the hypothetico–deductive method. I do think that Alcock is making a fair and constructive criticism in claiming that psi phenomena are not replicable in the strong sense of producing specific, reproducible findings. R & P meet this objection by putting forward three areas as claims for replicability. The focus thereby shifts from demonstrative findings under controlled conditions where experimenter or subject fraud is often, by default, the only "normal" explanation, to the collective findings of experiments whose significance levels are modest and whose methodologies were never intended to be the final ideal. I will limit myself to one of the areas chosen by R & P, namely, the ganzfeld. A short time ago, a known critic, Nils Wiklund, and I (Wiklund & Parker 1988) completed a survey of ganzfeld experiments. We arrived at findings not unlike those of Honorton and Hyman (1985) cited in both target articles. Our conclusions were nevertheless somewhat different: Even eliminating multiple analysis of results as a flaw (as they had later done), it was still possible to fault nearly all the crucial experiments on basic flaws such as permitting the possibility of sensory cues. Only a single experimenter's results would, in our opinion, satisfy the critic. So ironically we were then back to the issue of the whole case resting on experimenter integrity. Now it still seems to me improbable that all these flaws can explain the success with the ganzfeld technique, but in no way would I propose it as an established, replicable finding sufficient to convince the critic. What one might more reasonably hope to do might be to interest the critic in working with the technique. At this point, I would like to attempt to meet the challenge raised by Alcock.

What I read Alcock as requiring in order to legitimize parapsychology is that he be told how he (or other critics) should go about getting this so-called psi effect. The suggestion I now advance is not entirely based on my own vacuous opinion, but has its roots in a position paper circulated by the Parapsychological Association in 1986. It may well be that some of the necessary (but perhaps not sufficient) conditions for psi are found in subjects who have a definite belief in the phenomenon, who believe the experiment will work, and who are at least sufficiently extroverted to adjust with ease to the experimental conditions. It would also appear likely that persons who have an "openness to inner experience" (apparently one of the five major dimensions or surface traits in human personality), especially to dissociated states, make better subjects. Equal importance needs to be given to experimenters. Here the critic would be advised to have a role in designing the experiment and implementing safeguards, but to leave the running of the actual experiment to experimenters who have been chosen on the basis that they have enthusiasm, a positive expectancy that it will work, and an ability to relate to the subjects in a warm, emphatic way. If the area chosen for a replication study is ganzfeld, then obviously a sufficient number of experimenters must be included to allow for the estimated 40% success rate (P values being appropriately adjusted).

Many of these factors, as abstract as they may seem, can be measured at least to some degree by psychological tests, and I would be happy to cooperate with Alcock in the further design of such an experiment. I believe this suggestion also to be in harmony with R & P's request for cooperative research with critics. Such enterprises from critics are rare, but not entirely unknown. J. L. Kennedy, whom I mentioned earlier, reported in 1939 his attempt to replicate the Rhine findings with the use of 32 experimenters and 204 subjects. Although he interpreted his findings as "entirely negative," the telepathy series produced scores 3-million-to-1 against chance. Above-chance scores were contributed by 12 of the experimenters and 3 of the subjects. Unfortunately, the experiment would not be considered to have had adequate controls by modern standards!

Some suggestions from sociology of science to advance the psi debate

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The form that the critical debate between parapsychology and its opponents engenders seems to have become the only predictable thing about this area of human endeavour. The parapsychologists offer a mass of experimental evidence that prima facie seems to require us to take psi seriously; the critics, in response, tell us why this evidence is not as good as it seems. Experiments that are taken by one side to provide indubitable evidence for psi are challenged on grounds such as lack of repeatability, sensory cueing, or even outright fraud on the part of either subjects or experimenters. Often the critics' arguments are bolstered by a rather general set of considerations as to the characteristics of pseudoscience. Finally, lest anyone should forget its roots, parapsychology is deemed to be the home of closet dualists, spiritualists, or even occultists.

The debate between Rao & Palmer (R & P) and Alcock is typical of the genre. Although both sides appear to be mild and reasonable in the tone that they strike, the underlying issues have not changed. Psi will not lie down and die; neither will it stand up and be counted. On the other hand, the critics are not content to let an area that, according to them, has no real phenomena just fizzle away.

The trend over recent years in this debate has been for parapsychologists to play down their activity. According to R & P, parapsychology is no longer the revolutionary science – psi presents a mildly interesting, perhaps boring, rather small-scale set of irregularly produced anomalous events that with careful and increasingly refined study and with the aid of the latest experimental techniques may promise more. Indeed, the irony is that it is now Alcock, the critic, who makes psi seem more exciting, and it is he who points to the revolutionary potential of the field.

The dynamics of this debate have been documented before (e.g., Collins & Pinch 1979). Parapsychologists, by adopting many of the methods, procedures, and institutions of orthodox science, have been quietly attempting to metamorphose into real scientists. Their strategy is to show that in the study of psi "nothing unusual is happening." The strategy of the critics, on the other hand, has been to show that "something unusual is happening." After all, there is no point in spending a lot of effort knocking down something that is merely of marginal interest.

Reading R & P and Alcock reminds us how little progress has been achieved. On the part of the parapsychologists, the long-promised breakthrough always appears to be just around the corner, but somehow it never seems to have come. The critics also seem to have made little progress. They are locked into a symbiotic relationship with the parapsychologists, with each side being dependent upon the other for maintaining interest, but with neither side being able to achieve victory. Steady-state rejection has been the fate of this field for decades (Collins & Pinch 1979), and with the institutionalization of the critical forum in the shape of CSICOP (The Committee for the Scientific Investigation of Claims of the Paranormal), it seems likely that the impasse will remain. Certainly the target articles by R & P and Alcock seem to offer little that is novel in terms of a way forward.

Recent work in the history and sociology of science has shown that in their general form the arguments that parapsychology provokes are by no means atypical (Collins 1985; Shapin & Schaffer 1985). Even in the so-called hard sciences, during moments of controversy it is possible to find experimental evidence that is compelling to some scientists but contested by others. Studies of these controversies have shown that the interpretation of experimental evidence is rarely straightforward and that experiments are always embedded within a network of assumptions – some theoretical and some related to the practical contingencies of experimentation. The outcomes of experiments can always be contested by challenging one or more of the many assumptions upon which those outcomes rely. Subsequent experiments to test the assumptions called into question may not resolve the issue because they too rest on a further network of assumptions (Collins 1985).

R & P appear to refer to this issue when they point out that no single experiment in parapsychology need ever be decisive. However, their way around the problem – to suggest that the repeatability of many experiments will suffice – ignores the important point that deciding what counts as the "same" two experiments is itself dependent upon a set of assumptions that can in turn be challenged. Differences between, say, two ganzfeld experiments may for some purposes (for example, making an argument about the general repeatability of the ganzfeld technique) be insignificant; however, within another web of assumptions (such as those held by critics), such experiments can be seen to be crucially different (for instance, in the way they allow for possible sensory cueing). Thus protagonists such as Hyman and Honorton (1986) can reach different judgments as to the degree of repeatability for what is supposedly the "same" set of ganzfeld experiments.

Although arguments over the interpretation of experiments can be found in the hard sciences (Pinch 1986), such arguments are in most cases resolved fairly quickly by means of further experimentation and argument. Of course, as Planck has noted, some theories that have been superseded survive until their proponents die, but in most cases a consensus will emerge. Although criticism is always possible and any experiment can be shown to be defeasible, in most cases the process of criticism will come to an end. One feature of the parapsychology case is that such arguments appear to be interminable, and consensus never seems to emerge.

Are there any lessons to be learned from sociology of science as to how more progress could be achieved? Certainly one point to be made is that bringing together critics and proponents – such as Hyman and Honorton (1986) to investigate the repeatability of the ganzfeld – is unlikely to resolve anything. It is unlikely that proponents and critics will share crucial assumptions as to what counts as a competently performed experiment, and the outcomes of such joint projects are likely themselves to be indecisive (as appears to be the case with the Hyman–Honorton collaboration). Such collaboration runs the risk of merely serving to accentuate the debate – thus R & P, following Honorton, claim the ganzfeld is repeatable, whereas Alcock, following Hyman, finds the evidence on that score unconvincing.

The types of constraint that could be put on criticism in science in general have recently been considered by Collins (1987), who has offered two criteria that he suggests the arguments of the critic should meet:

A. Criticism should be "controlled" – that is, not equally applicable to mainstream science.

B. Criticism should be "controvertible," in the sense that a critic should be able to specify the conditions under which the criticised work could avoid the criticism.

An examination of Alcock's target article using these criteria is informative. His main criticisms of parapsychology are that (1) it lacks consistency in its findings; (2) its experiments have methodological and statistical flaws; (3) the evidence for particular effects falls apart under closer examination; (4) it has no experiments that are repeatable in the strong sense (i.e., reproducible by any researcher who follows the protocol properly); (5) its findings are unfalsifiable; (6) there is a lack of constraint on the appearance of psi that undermines its credibility; and (7) there is a lack of rapport with other fields of science.

It would seem that all of these criticisms bar one – criticism (3) – fall foul of Collins's two criteria. Criticisms (1), (4), (5), (6), and (7) fail to meet criterion A, and criticism (2) fails to meet criterion B. I will go through the reasoning briefly for each criticism in turn:

Criticism (1) also applies to many areas of social psychology where different patterns of data are looked for in different experiments, and where often the same data set is not relevant to the new experimental context. For example, Milgram's (1963) experiments on the amount of electric shock subjects were willing to allow to be administered to a victim demonstrate the phenomenon of obedience to authority, but this effect is not tested for in routine psychology experiments which often assume some kind of authority relationship between experimenter and subject. Criticism (2) is not controvertible because Alcock does not specify how particular experiments could be improved. Criticism (3) meets both criteria as it selects out particular experiments or series of experiments for examination. Criticism (4) fails to meet criterion A, because repeatability in this strong sense just does not occur in most science. Criticism (5) also does not apply in a straightforward way to most orthodox science. The crucial issue is not falsifiability per se, but which assumptions scientists should hold on to and which should be revised. For example the attempt to detect solar neutrinos from the Sun can be taken to have falsified stellar-evolution theory, but in practice that theory has not been abandoned (see Pinch 1985). Criticism (6) also applies to areas of mainstream science. Again in the case of the solar-neutrino puzzle, more than 400 papers have been published with proposed solutions, ranging from black holes in the sun to time reversal. There seems to be little constraint on such solutions (Pinch 1986). Criticism (7) would also seem to apply to many areas of orthodox science – branches of abstract mathematics, for example. This last criticism is of some interest, however, and I shall return to it below.

Thus it seems that in the main, Alcock's criticisms do not promote the advancement of the debate. However, the parapsychologists too could do more. Suggesting what they should do in detail would clearly require prescience and is impossible. However, I would like to suggest the general direction from which progress seems most likely to come.

Much has been made by critics of parapsychology's lack of a theory. This criticism can be sharpened – it is not so much a general explanation or a theory that is needed, but rather detailed low-level theories of the operation of psi in connection with particular experimental setups. Research in the sociology of science concerning the groups of experts who resolve experimental controversies has drawn attention to the crucial role of what might be called the "theory of the apparatus" in the process of replication (Collins 1985). Most experimenters in physics, when attempting to repeat an experiment at the research frontiers, do not build a carbon-copy experiment or what is known as a "me too" experiment. They try to improve on the apparatus such that the signal-to-noise ratio is increased. They do this with the aid of theories concerning the working of the apparatus. Of course, for a contentious phenomenon such theories may themselves come into question – but it is such theories that enable experimentalists to improve their designs. Thus, in attempting to repeat experiments, parapsychologists, rather than trying for carbon-copy repeats, should take a leaf out of the physicists' book and place a greater premium on boosting their signal-to-noise ratios.

A second suggestion I would offer comes from research that shows the importance to scientific progress of creating allies and building networks with other scientists outside one's immediate area of competence (Latour 1987). Of course, parapsychology has been trying to do this in a general sense by such moves as affiliating the Parapsychology Association with the AAAS (American Association for the Advancement of Science). However, something more concrete is required. It seems that the crucial thing is to try to engage the experimental and/or theoretical concerns of other areas of science in such a way that scientists in those areas must sit up and take note of psi. There seem to be two ways to do this. One is to get possible psi effects incorporated in a taken-for-granted manner within the protocols that are routinely used in other fields (Latour 1987; Pinch 1986). For example, orthodox psychologists need to be convinced that they should control for psi effects in the same way that they might control for experimenter effects. Another route would be to take standard experimental equipment from one area of science and show that because of psi effects that equipment does not work as expected, or needs to be modified. Again, scientists in that field would be more likely to pay attention to such developments, and it might be a way of bringing the debate over psi to a head. It is perhaps time for the parapsychologists to once more demonstrate that "something unusual is happening."

Psi: Anomalous correlation or anomalous explanation?

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Psychologist is pitted against parapsychologists; yet, on first reading, Rao & Palmer (R & P) appear more scientific than Alcock. It is Alcock who uses hyperbole[1] and guilt by association[2] to attack his opponents. Moreover, it is Alcock whose philosophy of science appears the more naive: "Sooner or later in science, it seems, the truth will out, and error falls by the wayside." Parapsychology has had its chance – and a fair one – he concludes.

By contrast, R & P seem to take a more measured empiricist line. They are simply sifting through a number of experiments and asking whether there has been credible replication of anomalous phenomena, without assuming that these anomalies will have parapsychological explanations. They plausibly argue that many of these experiments are at least as well-controlled as typical work in experimental psychology – how many psychologists have taken more than the most elementary steps in their protocols to protect against conspiratorial deception by subjects, or have demanded that null results be published alongside significant results with approximately the frequency of occurrence in actual testing? Alcock compliments those parapsychologists who show "circumspection" in focusing on anomalies rather than parapsychological explanations, but he then proceeds to call this "rare" and to attack psi research as a whole, not only on methodological grounds but also on very noncircumspect metaphysical grounds – suggesting that "the hidden agenda is the question of dualism," which is contrary to the (presumably scientific?) "materialistic, monistic" world view.

Three grades of psi. On second reading, however, I was struck that there may be a rather subtle way in which R & P's method may be less empiricist than it first appears. It all comes down to what is meant by "psi phenomena." I have no command of the literature, so I will simply consider the three characterizations I have encountered, taking into account also evidence afforded by textual usage. Hyman and Honorton (1986) claim that "psi phenomena" is a "neutral label denoting unexplained interactions between organisms and their environment," and add that "no particular explanation of the anomaly is intended, nor do we believe any is warranted at the present time" (pp. 352–53). R & P tell us that the term "psi phenomena" denotes "certain anomalous interactions that seem to involve psychologically meaningful exchanges of information between living organisms and their environment," where "anomalous" means "appear-[ing] to exceed somehow the capacities of the sensory and motor systems as these are presently understood." Krippner characterizes "psi phenomena" as "interactions between organisms and their environment (including other organisms) which are not mediated by recognized sensorimotor functions" (1977b, p. 2). (Alcock cites and appears to follow Krippner's usage.) I hope it is obvious that these characterizations are not equivalent and that they stand in a relation of increasing definiteness about what psi phenomena are – especially about what explanatory claims are involved in calling an observed phenomenon a psi phenomenon.

To help make this clear, let me introduce the notion of a *hec phenomenon*. A hec phenomenon is observed whenever, after the usual statistical and experimental controls have been deployed, a psychological variable shows a correlation significantly higher than would be expected by chance with some environmental variable, and there is as yet no agreed-upon explanation of the correlation. (Hec is a mnemonic for "*h*igher-than-*e*x-pected *c*orrelation without agreed-upon explanation.") Perhaps the correlation is between intentional button-pressings in one laboratory and the contemporaneous state of some card in another; or between deliberate willings-to-bend and subsequent bendings of nearby, but seemingly untouched keys; or between present verbal reports and difficult-to-predict future events. But the correlation might also be between verbal reports and sensory stimulation of any sort, as when I say, "That's a Dodge" in the presence of a well-illuminated Dodge, or when my reader says, "What does he know? – he's just a philosopher" in response to the arguments of this commentary.

Are there replicable hec phenomena? Of course. Because there exists nothing like a full, agreed-upon understanding of ordinary perceptual processes even in the simplest cases, experimental psychology is awash with hec. What has this to do with psi? Consider three notions of psi, meant to correspond to those mentioned above. Hyman and Honorton's (1986) psi just is hec, at least as long as we assume that the term "interaction" has no particular content – for example, if someone scores significantly better than chance in calling the outcome of a shielded coin-toss, this will be deemed a psi phenomenon even if the subject is not interacting with the *coin*, but merely enjoying normal relations with the local environment.[3] Call this *psi–1*. R & P's psi is more than this, not because they mention "interactions" and "exchanges," which I will again assume to be innocuous, but because they add that the hec phenomena "appear to exceed somehow" normal capacities, which the reports "That's a Willys" and "What does he know?" do not, though perhaps the other examples do. We will call a phenomenon *psi–2* if it is a hec phenomenon for which we think there is some substantial probability (say, > .25) that no explanation will be found among "the capacities of the sensory and motor systems as these are presently understood."[4] Krippner's psi is stronger yet, for, based on his characterization, it is not a matter of what sorts of explanations are plausibly in the running, but what the actual explanation is. So a *psi–3* phenomenon is a hec "not mediated by recognized sensorimotor functions," where this includes mere random variation.

Does this three-way distinction seem a bit unwieldy? If only things were so simple! For no sooner do Hyman and Honorton introduce their colorless conception of psi – psi–1 – than they begin to use "psi" to talk about psi–2 or psi–3. They ask, for example, whether "psi is responsible for the outcomes obtained in this data base" (1986, p. 353). This notion of "responsibility" seems to be causal–explanatory. They also say, after reporting their agreement that the ganzfeld experiments show an "overall significant effect," that they "continue to differ over the degree to which the current ganzfeld data base contributes evidence for psi" (1986, p. 352), suggesting that there is a distinction between, on the one hand, observing a correlation that survives various experimental controls and that is not at present explained, and, on the other hand, observing genuine psi. That would be possible if psi were psi–2 or psi–3, but not if it were psi–1.

Now the methodological empiricism of R & P depends upon assuming that they are concerned with psi–1, not psi–2 or psi–3. Yet their own definition suggests psi–2. Moreover, their discussion of Hyman and Honorton seems to show they conflate it with psi–1. For R & P begin by noting that Hyman was initially skeptical about whether the ganzfeld experiments "gave significant evidence for psi at or beyond the 5% level," and then go on to say, "Later, however, he came to agree with Honorton that "there is an overall significant effect in this data base which cannot reasonably be explained by selective reporting or multiple analysis (Hyman & Honorton 1986)." Hyman's initial claim, I take it, was that experimental controls, elimination of selective reporting, and so on would make the apparent hec go away. He later came to agree with Honorton that they did not – that is, that a statistically significant correlation remains even after various sources of potential bias have been controlled. Does this show that he came to agree that there is "significant evidence for psi at or beyond the 5% level"? If psi–1 is in question, yes. But it is clear from R & P's usage throughout the paper that they have in mind psi–2 (as in their initial characterization, quoted above), or even psi–3 (as when they talk of "psi information," "psi receptivity," "psi tasks," conditions "conducive to the manifestation of psi," "techniques of eliciting and measuring psi," and "the reason that psi has not yet been applied on a broad scale"). Alcock and R & P alike manage to cite only those conclusions of Hyman and Honorton (1986) that serve their own purposes, while omitting to mention the important conclusions that do not. But R & P compound the problem by embedding their discussion in a paragraph that seems likely to lead the reader to believe that Hyman and Honorton have reached agreement that there is "significant evidence for" psi–2 or psi–3 rather than just for psi–1, despite their explicit disavowal of any such claim (Hyman & Honorton 1986, p. 352).[5]

Conclusion. If psi is psi–1, then there is no need to ask psychologists to take psi research seriously – most psychological research is already psi research, so what the heck? If psi is psi–2 or psi–3, then does the appearance of hec in, say, the ganzfeld data base give evidence for psi? Only a very small amount. Consider first psi–3: To describe a hec phenomenon as an instance of psi–3 is to advance a general hypothesis to the effect that the explanation of the hec is something beyond "recognized sensorimotor functions." There is one salient competing explanatory hypothesis – namely, that hec occurs as the result of "recognized sensorimotor functions" (including random variation), although we do not yet know what this explanation looks like. Both hypotheses fit the ganzfeld data, and so neither is "rejected" by them; both assign the data the same likelihood. But the latter hypothesis has much higher antecedent probability, for it is consistent with and draws upon the resources of well-confirmed theories in physics, biochemistry, and psychology, whereas the former hypothesis seems likely to require substantial revisions of or supplements to these theories. This "antecedent probability" is not an a priori probability, and so not a "mere bias" in favor of "normal science." Rather, it is the result of a long history of surprisingly successful theorizing and experimentation, with some substantive and methodological meta-inductions thrown in. If this is "mere bias," then so is science of any sort, and no "unbiased" empirical theory of any consequence could ever be arrived at inductively.

For the same reason, the hec present in the ganzfeld data base does not do much for the credibility of psi–2, for the salient competing hypothesis is the claim that, although we do not now *have an explanation of the hec in terms of recognized capacities*, it is more probable than .75 that the explanation, when found, will be in those terms. Both hypotheses fit the data, but again, the one is antecedently very much more probable than the other.

The observation of hec in ganzfeld experiments and others like them thus certainly contributes evidence to psi–2, psi–3, and even to parapsychological hypotheses. My uninformed view is that psychologists should admit this and resist impugning motives. It is enough to say that these hypotheses are antecedently not very credible, and that their increase in credibility given the (in some instances, no doubt genuine) observations of hec that R & P cite is slight. At some point, presumably, repeated observation of hec in such experiments, along with exhaustion of the most plausible alternative explanations within the range of recognized mechanisms, and along with emergence of a predictively successful theory postulating heretofore unrecognized mechanisms, should undermine our confidence in going theories and significantly alter antecedent probabilities.[6] This is the familiar route by which anomalies in the past have precipitated significant theoretical changes elsewhere in science. In the present state of play, however, it would seem that an assessment of antecedent probabilities capable of making psi–2 or psi–3 significantly probable on the evidence invoked would involve a departure from "normal" values of much greater magnitude than the amount of unexplained correlation in the experiments discussed by Rao & Palmer. It would take a paranormal probability distribution to make it credible on such a slim basis that paranormal phenomena have been observed.

NOTES

1. For example, Alcock claims there is an "'anything goes' attitude in parapsychology" and decries "the lack of any constraints on the appearance of psi."

2. "Most religions teach that the Soul survives death in some form. The question of survival of the parapsychologists' 'soul' or 'mind' or 'personality' after death is, even many leading parapsychologists agree, an important question for parapsychology to consider. . . . Blackmore . . . suspects that just as it was the *fundamental* question to many of the early psychical researchers, it is still so for many of her fellow researchers today" (Alcock, sect. 7, para. 6).

3. If this assumption is not made, then the term 'psi' will of course be very far from "neutral," and hence that much less able to serve its intended function.

4. Here I am assuming that R & P are using the qualification "presently understood" to modify the *capacities* of the systems, rather than the systems themselves. If we were to read them as saying no more than that a psi phenomenon is a hec phenomenon that we do not at present know enough about the sensory and motor systems to explain, then this would simply be psi–1.

5. A further difficulty is a looseness in R & P's language (sect. 4.1.2, para. 3), which invites confusion of testing for statistical significance (which has its methodological foundation in a nonconfirmational view of theory-testing) with trying to establish "significant evidence" (which suggests a confirmational view).

6. Does this mean that "the truth will out" after all (cf. Alcock, sect. 6, para. 4)? – Only if appropriate experiments are attempted and appropriately carried out, and appropriate hypotheses and theories are conceived and developed, and the community of scientists appropriately conditionalizes a consensual credence function, etc. Neither general features of "the scientific method" nor historical features of the actual community of scientists guarantees such conditions. For want of experimental effort or ingenuity, or lack of theoretical imagination, or inability to overcome preconceptions, a true parapsychological theory might go undiscovered by science.

Are there any "communications anomalies"?

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What is *most* interesting about this pair of target articles concerns the general state of the debate more than the specific claims or specific lines of argument contained therein. Most of what is said here has been said before. What is perhaps surprising (especially to those of us who are inclined to doubt that there is anything unusual going on in the "psi" experiments) is that Alcock is not able to make a stronger case and that Rao & Palmer (R & P) are as reasonable as they are.

In these brief remarks, I will first address some specific problems in the two target articles offered here. These are indicative of more general problems that plague the larger debate. Because such problems are rather typical of scientific conflict, I will address general problems of assessment in a second section. In a final section, I will make some comments about the future of this debate, focusing attention on recent contributions by Hyman and Honorton.

1. Talking around one another: Alcock. Alcock quite obviously has a pet peeve. He is concerned with calling attention to the extent to which mind/body dualism may be a "hidden agenda" in much "parapsychological" work. This is all very nice, but it makes it difficult to see how his arguments can be brought to bear against those who, like R & P, explicitly renounce such an agenda, hidden or otherwise. They want to treat psi phenomena as anomalies. The question for them is whether there are such anomalies.

Surely that is the right question. Hyman and Honorton (1986) have recently urged that the term "psi" be understood only as denoting a "communications anomaly," without intending any particular explanation. R & P have tried to observe this convention. Alcock has not.

Alcock uses the terms in such a way that *either* the phenomena in question are "paranormal" (i.e., to be explained in a way that transcends the normal parameters of explanation) *or* they are "based on error and self-delusion." This rules out, *a priori*, the possibility of *natural* phenomena which, although as yet inadequately documented, are really there and explainable in natural ways. X-rays are examples of physical phenomena that at one time had this status, and the history of the discovery of x-rays is illuminating in this connection.

Alcock fails almost completely to address the arguments of those who wish to treat psi phenomena as mere anomalies, except (1) to mention that such people exist and (2) to challenge them to "consider just how they are going to define their subject matter without some reference to the independence of the mind from the materialistic realm." This ignores much of the literature that Alcock himself cites, notably materials written by Palmer and by Honorton, and it ignores a perfectly acceptable analogy that Alcock himself mentions: The navigation of bats was at one point an anomaly, but was later shown to involve a special physiological capacity that had been hitherto unobserved. It does not take a parapsychologist to see that the discovery of such "natural" explanations for psi phenomena would be welcomed by those who are psi advocates, and that Alcock's challenge is thus easily answered.

Alcock cites Gardner Murphy's (1961) argument that "even if the paranormal were to be defined only in terms of anomaly, this would still lead to a dualism of some sort because of its independence from considerations of time and space." In the first place, it is not clear that all psi phenomena (understood as anomalies of communication) require such independence; in the second place, even if they did, it is not clear how mind/body dualism is supposed to follow from such independence. I hope Alcock can clear this up in his reply.

Alcock's special preoccupation with dualism (or spiritualism) makes it difficult, then, to know what to make of his claim that "parapsychologists have clearly failed to produce a single reliable demonstration of 'paranormal,' or 'psi,' phenomena." Does he mean that no otherworldly souls have been demonstrated? Surely he is right. Does he mean that no "communications" phenomena have been found whose explanations are not yet at hand? Perhaps he means to say this, too. But that conclusion is not supported by his argument, which seems directed mostly at the former claim (one not made by R & P).

Finally, Alcock asks why parapsychological experimenters have not set out "to try to produce in subjects the subjective impression of telepathy." This is in some ways a fair question, but it is hard to think of very reliable ways of doing this. It is because "subjective impressions" are so very hard to work with that experimenters have resorted to rather artificial experimental setups, whether in perceptual studies or in parapsychological ones or whatever. These may, as Alcock suggests, be unsatisfactory for a variety of reasons. What is examined in the laboratory may have only a remote relation to the phenomena originally inspiring the scrutiny. But it seems that the *best* course for someone like Alcock (as was true of J. J. Gibson (1979), who was similarly critical of classical perceptual experiments) would be to take upon himself the job of designing an adequate experiment. I think he knows that this would not be an easy matter if it is to involve things like the "subjective impression of telepathy."

In the long run, Alcock is right when he suggests that hypotheses ought to be falsifiable, where this is possible. It is for that reason that he ought to put himself in a position where his own views have some chance (however remote) of being falsified.

2. Talking around one another: R & P. At one point, Alcock appears to *approve* of "circumspection" like Palmer's. Indeed, R & P seem largely to pick their way cautiously through the many possible traps at hand in the area of parapsychological research. Nevertheless, there are several remarks worth making concerning their target article.

R & P argue that "potential explanations of psi that violate the basic limiting principles of nature ["omegic" explanations] are scientifically legitimate and, along with conventional explanations, should be entertained from the outset in our efforts to explain psi anomalies." R & P are, of course, right about the *legitimacy* of such things; but how will they respond to the claim that, on the other hand, it is perfectly legitimate to *resist* such out-of-bounds explanations? Although "open-mindedness" is perfectly virtuous among scientists, so is some appreciation for the soundess of what has so far been established. A kind of conservatism among scientists regarding the merits of the way they understand the world is not only understandable, but unavoidable. Throwing away a perfectly good theoretical framework (or "paradigm" or "scientific research program") in the absence of a suitable substitute is not the mark of a wise researcher. Thus it seems most reasonable to anticipate that people will seek out "natural" explanations for anomalies and that their resistance to "nonnatural" explanations will be fairly strong.

On the other hand, however, this is not to say that those who embrace the notion that psi phenomena are not explicable in conventional terms need for some reason to be deterred on such "conservative" grounds. Quite the contrary: They need only satisfy their *own* theoretical scruples. But they should not be surprised if, in the absence of truly *dramatic* argument or evidence, others are more resistant than they are.

R & P claim that "some critics of psi research have demanded a 'foolproof' experiment that would control for all conceivable kinds of error." I know of no such critics. Instead, critics have been suspicious (a) of particular experiments, and (b) of an extraordinarily annoying "tendency" to sloppiness among parapsychological experimenters. Is it true that these experiments tend to be less rigorous in their controls than experiments in psychology generally? If such a tendency is there, how is it to be explained? What's going on here? Where clearly enunciated suspicions have been voiced, they must be addressed one by one.

3. Assessment of scientific research. Alcock offers a very nice, brief review of historical cases of curious claims in science and their reception. As he observes, the reception is usually cool, but this does not mean that the claims might not eventually gain acceptance. Often they do. Because of a degree of conservatism at this level, science manages to achieve some stability, which makes the eventual overthrow of accepted points of view all the more impressive. Alcock's discussion of this case is right on the mark, and is clouded only a little by his excessive emphasis on spiritualism.

In the light of this kind of reasoning, however, one must be cautious in how one applies considerations such as "an extraordinary degree of evidence [for] . . . extraordinary claims." This is surely the way the matter will be perceived by those who take themselves to be in the mainstream, and (as mentioned above) people who are doing "extraordinary" research should expect nothing different from those whose views they are arguing against. But mainstreams do change. However it is accomplished (and there is, of course, a great deal of argument about this), it has happened many times that where once a view was widely regarded as highly improbable, it later became part of orthodoxy. Whatever else may have been going on in these situations, it is clear that such "revolutions" would never have occurred had the ostensibly heterodox researchers and theorists heeded the advice of their establishment colleagues.

What emerges fairly plainly is that arguments about burden of proof probably ought to be ignored by all sides. It is perfectly appropriate for those who are enthusiastic about apparent anomalies to continue to try to expose them and investigate them, and it is just as appropriate for those who are unimpressed by results so far to explain why they are unimpressed.

If there is to be wider acceptance of psi phenomena as anomalies, however, there will surely have to be more dramatic evidence. It is the burden of researchers to try to design clear and dramatic tests, as decisive as possible, of the phenomena. It seems to me R & P are right in thinking that everyone would benefit from wider participation of critics in the experimentation, but the problem is to convince such critics that the field deserves such effort in the first place. Dramatic results would accomplish this (dramatic results are especially attractive to skeptics, for obvious psychological [and logical] reasons; the skeptic needs to try to debunk these things), but it seems to be a sorry fact that when dramatic results have been publicized (and outsiders have in fact been attracted to the research), the eventual outcome has not favored the thesis that "communications anomalies" are present.

But as is implicit in Alcock's brief survey of history, even if the main motive in pursuing a line of research were religious or metaphysical, this is not sufficient grounds for condemnation. Such commitment has been known to yield important scientific results. The proof is in the testing.

4. The present and future state of the debate. Things are not so bleak, however, as may be indicated in the two accompanying target articles or in the remarks above. This is because an extraordinarily valuable contribution to the discussion has been made by Hyman and Honorton in the articles referred to in both of the target articles. Hyman and Honorton began their contribution by taking each other seriously in an exchange of papers in 1985, and they have most recently offered a jointly authored piece (Hyman & Honorton 1986) in which they try to clarify not only their disagreements but also their common ground. Perhaps what is most important about this last contribution is the set of recommendations they make concerning procedure, which should be required in all future ganzfeld experiments.

As Alcock says, "judgment should be suspended until there is at least some consistency among research findings from a body of methodologically irreproachable experiments, at least some of which are repeatable in Beloff's . . . strong sense." The procedures outlined by Hyman and Honorton should take some pretty big steps toward satisfying Alcock in this matter.

In the last analysis, researchers in this area must encourage participation – and even criticism – from people who are not sympathetic. If researchers want to know what it is that they must do to convince skeptics, the answer is that they must answer the particular objections raised by the skeptics – at least when such objections involve corrections in experimental procedure that can be made reasonably easily. But skeptics cannot expect that they can get away with untestable complaints. They, too, ought to clean up the experiments and perform them, for if they perform the experiments without the mistakes but still get the results obtained by those they have criticized, we will all learn a great deal. To the extent that this has been done by critics, the debate has been healthy. To the extent that it has not been done, it has been a war of near-religious commitment on both sides. When the experiments are performed by a wider variety of researchers using more tightly controlled techniques, we will then see how this chip falls.

Alcock's critique of Schmidt's experiments

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Alcock's discussion of my work contains factual errors and misleading statements that might have been avoided by a more careful reading of Schmidt (1969b), the paper that forms the center of his arguments.

Schmidt (1969b) describes the provisions against subject fraud, such as the automatic double recording by external punch tape and internal nonreset counters. Subject fraud would have required, apart from specific electronic knowledge, much undisturbed time for opening the bottom plate of the test machine and feeding in electric pulses in order to fool the internal counters as well as the external recorder. My paper also reports that I was personally present in all tests, with the exception of a small part of the sessions with one subject, and that the scores in these few sessions were not higher than the other scores. In none of my subsequently reported experiments was there any less stringent subject supervision. Nevertheless, Alcock flatly states that "subjects were usually unsupervised."

Due to editorial restrictions, there was no space in my paper to go into the details of the randomness tests. The paper indicates, however, in footnote 4, that a detailed report on the random generator and the randomness tests is available (Schmidt 1969c). This report answers in particular Alcock's question about the frequency of 4s in the different experiments: Only in the part where a subject was trying to enforce the generation of 4s (lighting of the red lamp) did the generator produce a significant excess of 4s.

The report provides an additional argument against temporary preferences of the machine for one target. A sequence of more than 4 million random numbers (1, 2, 3, and 4) had been collected from the machine in automated runs made between the sessions with the subjects. The frequencies of the individual events as well as of the next-neighbor correlations are listed on page 17 of the report. These numbers give no indication of nonrandomness. If the machine had a tendency toward a temporary bias for 4s, this should have led to an increased total count of 4s. And even if the bias occasionally shifted between the four outcomes, this should be evident in the correlation matrix.

If the random generator had been a "black box" of unknown structure, it would have been desirable to extend the randomness tests to higher correlations. From the known, simple structure of the generator, however, one can argue that a malfunction leading to nonrandomness must already lead to anomalies in the counts of single events or next-neighbor correlations.

Alcock's statement that I frequently changed the components and the design of my random generator also needs some comment: My random generator uses the random timing of radioactive decays as the basic source of randomness. A rapidly advancing binary counter (1 million steps per second) is stopped at the random arrival time of a signal from a Geiger counter. Then the lower bits of the stopped counter provide practically ideal random numbers. The randomness features of this generator have been discussed in detail (Schmidt 1970d). Because of its reliability and conceptual simplicity, I have used this type of random generator for nearly all of my experiments. Following the progress of technology, the original circuit elements (not even manufactured anymore) were naturally replaced by more modern, integrated circuits that simplified the construction and maintenance.

Alcock's feeling that my work lacks continuity – that I often "move on to a totally different situation" without refining the present measurements – may be more understandable. What needs to be done first, and what can be considered as a logical next step, depends much on personal taste and educational background.

I certainly do not see this work as Alcock does: an "attempt to establish the reality of a nonmaterial aspect of human existence." For me, the underlying questions are very specific: Can we produce experimental evidence against a universality claim of current physics, in the sense that present physics gives the best possible description, even of systems that include human subjects? And if so, can we specifically say which of the conventionally accepted laws of physics fail in such systems, so that we can provide a solid foundation for a future theoretical framework?

Even in a research effort that is very well focused, there will be some side roads. We will want to explore, for example, other random generators and other forms of feedback to be reasonably sure that we don't overlook other, possibly more efficient, approaches to psi testing. On the other hand, we have to be selective because each study takes much time and effort. It therefore often seems more reasonable to postpone the study of some details until we have pursued the main questions that should contribute most to our understanding of the overall picture. How my many experiments fit into the search for the overall picture may be seen best from review articles (Schmidt 1977; in press).

Psi: Repeatability, falsifiability, and science

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The target articles by Alcock and Rao & Palmer (R & P) are the latest installments in a controversy that stems from at least as far back as the late eighteenth-century investigations of the "higher" phenomena of mesmerism (Leahey & Leahey 1983). In our opinion, Alcock is quite right in concluding that the available data provide no good evidence for the existence of a psi effect. Despite what R & P contend, there are simply no psi effects that have proven replicable in the laboratories of skeptics as well as of believers under well-controlled conditions. R & P attempt to support the psi hypothesis by referring to the results of meta-analyses that demonstrate overall above-chance scoring. They even quote Hyman (Hyman & Honorton 1986) out of context to make it appear as if he too has come to give credence to the combined results of ganzfeld experiments: "Later, however, he [Hyman] came to agree with Honorton that 'there is an overall significant effect in this data base which cannot reasonably be explained by selective reporting or multiple analysis'" (sect. 4.1.2, para. 3). However, what Hyman (1985a; see also Akers 1985a) in fact pointed out was that the individual experiments that went into the ganzfeld meta-analysis were often flawed in ways that could have produced above-chance scoring without psi (e.g., failure to properly randomize stimuli, opportunities for sensory leakage). It is unclear to us why R & P believe that a collection of badly done experiments is more likely than a single badly done experiment to convince skeptics that subjects can actually read the thoughts of a sender located on the next floor.

Akers (1985a) suggested that meta-analytic techniques might prove useful in future psi research if believers and skeptics can agree before the research begins on a set of methodological standards that will be adhered to across laboratories. However, the retrospective application of meta-analysis to studies that are controversial on methodological grounds will do nothing but convince skeptics that believers are both credulous and sloppy about experimental procedures.

Despite our basic agreement with Alcock, we believe that he sometimes weakens a good case by attacking parapsychology on unfair grounds. Alcock is surely right in arguing that much parapsychological research has stemmed from dissatisfaction with materialism as a worldview. However, from the title of his paper onward, he assumes that such a motivation on the part of parapsychologists *ipso facto* makes their endeavors scientifically suspect. In reality, much of the highly respected work of such eminent scientists as Kepler, Newton, Flourens, James, and Sherrington (the list could be easily extended) was motivated by dissatisfaction with materialism (Koestler 1959; Leahey & Leahey 1983; Sherrington 1951). Both Flourens and Sherrington, for example, believed that their respective neuropsychological findings provided evidence for the existence of a nonmaterial consciousness. The empirical findings of these scientists have not stood or fallen on the basis of their beliefs about materialism, and neither should any empirical findings generated by parapsychologists.

Alcock correctly points out that parapsychological notions are unfalsifiable and that parapsychologists invoke *post hoc* principles to explain away the failures that contradict their views. However, the same is true of well-established, conventional scientific paradigms. Parapsychological notions are no more unfalsifiable than is the theory of evolution (Feyerabend 1978a), [see multiple book review of Grünbaum's Foundtaions of Psychoanalysis, *BBS* 9(2) 1986] psychoanalytic theory, cognitive dissonance theory (Tetlock & Manstead 1985), or "special state" theories of hypnosis (Spanos 1986). The Popperian notion that "real" science and pseudoscience can be separated by the falsifiability of the former is simply not valid (Feyerabend 1978a).

Established scientific theories maintain their unfalsifiable status because proponents are adept at inventing *post hoc* mechanisms and processes that "explain" findings that appear to contradict the theory. For example, many experiments have found that nonhypnotic control subjects respond just as well to suggestions as hypnotic subjects do (cf. Spanos 1986). Believers in hypnosis did not respond to these findings by abandoning the idea of a hypnotic state that facilitates responses to suggestion. Instead, they simply invented the theory-protecting notion that control subjects, unbeknownst to the experimenter, inadvertently slip into hypnosis (Spanos 1986). How long and to what extent *post hoc* inventiveness of this type continues before one theory is replaced by another is dependent on a host of complex factors: the utility of the original theory at generating interesting new findings, the explanatory potential of competing theories, the relative political power in the scientific community held by the proponents of the competing theories, and so on. Clearly, however, theories are never stripped of (or initially denied) the coveted label "scientific" simply because they are unfalsifiable or because their proponents are adept at inventing *post hoc* protective principles.

Will the assumptions of contemporary science really be threatened if a reliable psi effect is discovered? It is hard to see why. Suppose we discover a meditation technique that reliably enables our subjects to levitate. At first (sensibly) no one believes us. However, we persevere, skeptics replicate the effect in their own laboratories, and fraud is eliminated as a viable hypothesis. Our guess is that such a discovery would lead to a bevy of studies aimed at delineating relevant antecedent and mediating variables and to all sorts of hypotheses (mostly wrong) aimed at making sense out of the phenomenon. In other words, levitation would come to be treated by established science like any other initially surprising but interesting phenomenon. Initially, this phenomenon would challenge everyone's commonsense expectations about how things operate in everyday life. As far as we can see, however, it would not challenge the basic assumptions of contemporary science. In fact, it could not do so because what those assumptions are remains obscure. It is accordingly unclear how any reliable finding could, in principle, violate them.

Parapsychologists do not have a science because they simply do not have a phenomenon to build a science around. Oddly, however, critics sometimes think it necessary to go further and argue that parapsychology constitutes a kind of revolutionary threat to the established scientific order. Because of the havoc they may wreak, it is argued that parapsychologists should be forced to meet particularly stringent methodological standards before their work is accepted by established science.

One problem with such arguments is their implication that the work of parapsychologists already meets the less stringent methodological standards that apply to those of us who work in more conventional areas. Critical reviews of the parapsychology literature make it abundantly clear that this is not the case (e.g., Akers 1985a; Hyman 1985a). There is simply nothing extraordinary in expecting parapsychologists to (a) provide data concerning control trials of adequate length for a random-number generator (cf. Hansel 1981), (b) eliminate informational cues in remote-viewing studies (cf. Marks & Scott 1986), (c) insure proper randomization of stimuli (cf. Akers 1985a), (d) insure that stimulus cards are not transparent (cf. Hansel 1980a), and that ganzfeld experiments will not provide opportunities for "sensory leakage" (Hyman 1985a), and so on. There is also nothing unusual about scientists in conventional areas failing to take seriously dramatic findings that cannot be replicated in independent laboratories.

If parapsychologists ever develop a reasonably careful methodology that yields results that can be replicated by any careful and knowledgeable investigator, they will be meeting the same basic standards met by investigators in conventional fields of experimental science. That hardly seems too much to ask.

The status of parapsychology

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Alcock seems to be characterizing parapsychological views as uniformly endorsing claims such as the distance- and time-independence of anomalous communication and its so-called goal-oriented character. He has neglected to say that parapsychologists have often debated such claims and have tried to examine and refine them empirically. Parapsychologists are far from univocal in interpreting the events of concern. In recent years, parapsychologists seem to be showing, both conceptually and in their research, increased awareness that the viability of psi research as a scientific endeavor depends on the ability to discover some kinds of boundary conditions for the effects. Two recent studies that are particularly germane to delineating boundary conditions for parapsychological effects are those of Schmidt (1985) and of Vassy (1986). They concern, respectively, temporal and complexity limits of parapsychological effects. There is also recent work published in traditional psi-research journals that invokes some quite prosaic physical constructs, such as electromagnetic energy (e.g., Persinger 1987; Persinger & Cameron 1986). Parapsychologists are now very active in making explicit the assumptions behind their work and in questioning even some of the very fundamental ones, such as the assumption that alleged PK (psychokinetic) studies using pseudorandom-number generators actually measure a PK effect, rather than "intuitive data sorting" in which subjects select favorable times for responding in such a way that outcomes will match targets (May et al. 1985; Radin & May 1986). Researchers are now asking fundamental questions about the events being studied and are doing so in some conceptually sophisticated ways. They are also investigating the contrasting implications of particular models. This is the first time these things have happened on this scale in parapsychology.

Alcock would like to see investigators focus on the psychological basis of anomalous experiences. This worthwhile goal can, however, be pursued simultaneously with examining whether there is any truly anomalistic element in such experiences.

Parapsychologists must become more deeply involved in unraveling the psychological aspects of anomalous and altered-states experience and of the ESP-task setting if they are to advance psi research (Stanford 1987). The historical disjunction of parapsychological and psychological research might have been an important factor in the slow progress of parapsychology, but attitudes are changing. Parapsychologists are now showing a much higher level of interest in contributing to psychological knowledge. Some examples related to out-of-body experience (OBE) are the works of Cook and Irwin (1983), Myers et al. (1983), and Irwin (1985c). Systematic studies of the psychology of the ESP-test situation (ganzfeld, specifically) have been done by Stanford and Angelini (1984), Stanford et al. (1985), and Stanford and Roig (1982).

Alcock, who seems to have relied on secondary sources, apparently misunderstands the nature of research on the so-called release-of-effort effect (henceforth, ROE effect) in PK research. He apparently believes that parapsychologists casually observe some odd things in the outputs of their PK-test apparatus after a session has ended and take these as serious evidence of PK (ignoring the times when it does not occur). The fact is that both nonlaboratory anecdotal material and unanticipated laboratory observations led to the creation of an hypothesis – not an unusual sequence of events in science! – concerning the occurrence of PK effects after the cessation of egocentric effort aimed at making the PK goal-event occur. This hypothesis has subsequently been subjected to deliberate study by various researchers under specially designed circumstances. This is not the place to review the evidential status of this hypothesis, and no specific claim is being made for it here. Examples of post-effort effects certainly exist in cognitive psychology. The documentation of similar effects in PK work could help forge links with cognitive psychology (see, e.g., Irwin 1979). The main point here is that the serious evidence related to the release-of-effort hypothesis is certainly not based on *ad hoc* interpretation of serendipitous results.

The quality and tone of external critics' remarks are, in my own judgment, becoming generally more reasonable and temperate of late. Perhaps so, too, are parapsychologists' statements. If this perception is correct, we may be able to look forward to the prospect of some real dialogue.

I hope that both parapsychologists and other scientists will *not* adopt the Rao & Palmer's (R & P's) term "omega" to "identify potential explanations of psi that go beyond the basic limiting principles." There is a long tradition of using the term "omega," the final character of the Greek alphabet, to indicate an ultimate principle or being, or a final end result. Such terminology is found both in scripture and in theological writings. Through recent overuse, the word "omega" is even becoming a bit hackneyed. The term could load parapsychology down with imagined and undesired connotations related to religion, spirituality, and other "ultimate things" (as suggested by the term "omega"). Fortunately, this term has not become widely accepted within the parapsychological community. I mention it here in the hope that it will not become accepted by scientists outside that community either.

I am not sure that scientific progress is served by creating a neologism to designate interpretations of "psi" phenomena that allege that the phenemona "go beyond the basic limiting principles." When such ideas are developed, each should be considered on individual grounds, both logically and empirically. Lumping them together through a neologism, especially under a vague, mystical-sounding term such as "omega," might unfairly cause them to be collectively dismissed as so much metaphysical balderdash. What some persons think such theories have in common may be less important than how they differ, and it is here that I am very concerned, for we already see evidence, as in Alcock's paper, that outside critics can easily come to think of parapsychology as far more monolithic conceptually than it is or ever has been! I also question the usefulness of lumping together explanations with regard to whether they are believed to violate alleged "basic limiting principles." It seems to me that the only intrinsically sacrosanct principle in science (other than the need for honesty and accuracy) is the one that allows the scientific method to operate; in essence, it is the principle that there are boundary conditions for all circumstances. (Less stuffily put, scientists must believe that "not all things are possible.") Talking loosely about "limiting principles" may simply disguise and ennoble prejudices. A useful distinction is one differentiating "theories" that indicate testable boundary conditions for so-called psi events from those that do not. (The latter, however, are not really scientific theories in that, obviously, they are untestable.) Another useful distinction is between theories that propose changes in, extensions of, or additions to current scientific constructs in order to explain "psi" observations and those that explain them on the basis of currently accepted principles. The two relevant dimensions here are designated by the neutral terms *testability* and *conventionality* of theories. Is it really helpful to coin more new terminology?

R & P state that a "wide range of research seems to converge on the idea that, because ESP 'information' seems to behave like a weak signal that has to compete for the information-processing resources of the organism, a reduction of ongoing sensorimotor activity may facilitate ESP detection." Presently however, the experimental literature provides no basis for preferring this noise reduction hypothesis over several other proposals, perhaps equally cogent ones, that theoretically implicate other factors. It is gratuitous to favor one of these competing hypotheses over the others at this stage of gross ignorance, unless one does so strictly on conceptual or esthetic, rather than empirical, grounds. Fortunately, R & P note that "more research will be needed before the status of the noise reduction model can be conclusively determined." Unfortunately, they go on to suggest that a "large body of empirical data . . . is nevertheless consistent with this hypothesis," without also indicating that it is also consistent with other explanations and that specific, unique deductions from the noise reduction hypothesis have not so far been clearly supported by research or even examined through systematic experimentation. The noise reduction hypothesis is elegant, simple, and attractive. It may or may not be the sole correct or even a correct explanation of the relevant observations. A chapter in a recent book discusses these matters at length (Stanford 1987).

Based on circumstances that are apparent from the R & P paper, I would suggest one possible explanation for the lack of progress in parapsychological theory development. One requirement of theory development is systematic, hypothetico–deductive research. It could, in part, be the paucity of such work that has held back parapsychological theory development. Despite considerable evidence (see R & P's paper) that ESP-task success is associated with the use of particular procedures (e.g., ganzfeld), with personality traits such as extroversion and freedom from neuroticism (or defensiveness), and with cognitions such as belief in the possibility of ESP occurring in the test situation, in none of these cases have researchers developed a meaningful body of empirical evidence indicating why these circumstances relate to ESP-task success in these particular ways. There has been little or no experimental or other empirical work on hypotheses intended to explain widely discussed central findings within parapsychology. The lack of such work might be related to parapsychology's historical need to defend itself against allegations that its findings are artifactual.

Finally, I wish that Palmer had had more opportunity to elaborate on his ideas about the undesirability of defining psi functions negatively and on the importance of developing testable constructs related to anomalous communication (e.g., Palmer 1985c). (See also Stanford 1974.)

Rao & Palmer are to be commended for a clear, cogent explanation of their position and, generally, a very thoughtful, well-organized paper.

Is searching for a soul inherently unscientific?

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As a psychologist with more than 25 years of practical experience in parapsychological research, I find Rao & Palmer's (R & P's) target article to be an accurate and informative summary of contemporary knowledge of psi, although it adopts a more conservative stance than I would have taken. Particularly important in their review is the emphasis that, at this stage of our knowledge, psi phenomena are psychologically delicate and only statistically replicable, rather than robust: Currently unknown changes in the psychological conditions of an experiment, or in an experimenter's attitude, can make psi appear and disappear. Alcock's target article, on the other hand, reads well if you have little first-hand knowledge of parapsychology, but it is actually misleading and inaccurate.

Space limitations in these commentaries would not allow me to attempt even a minimally adequate correction of Alcock's many misleading statements, although I assume other commentators will deal with some of them; the interested reader can go back to the original research literature. I want to concentrate instead on two aspects of the philosophical position, only partly explicit in his article, that Alcock apparently brings to his analysis.

Why all the fuss? If we are indeed so certain of our scientific knowledge that we can a priori declare that parapsychological phenomena are almost certainly unreal, why do Alcock and other critics make so much fuss over it? Looked at in terms of consumption of scientific resources, parapsychological research is a trivial activity: My 1978 survey showed only half a million dollars a year in support for *all* the parapsychological research in the United States (Tart 1979). Estimating today, I believe it operates on less than 4 million dollars per year and employs hardly more than two dozen people full time in the entire non-Communist world. So what's the fuss? Why should people devote great time and effort to criticizing the research, thus creating a climate in which future parapsychological research is inhibited?

Alcock might object that I am illegitimately raising an issue about the motives of the critics of parapsychology. As Rao & Palmer (R & P) point out, ad hominem attacks on people who disagree with you are not a legitimate part of science. My observations as a psychologist (Tart 1982; 1984; 1986b; Tart & LaBore 1986), however, have been that parapsychology is generally not an emotionally neutral topic and that the motives of *both* "believers" and "nonbelievers" must be examined and formalized into testable hypotheses if we are to fully understand attitudes toward and manifestations of psi. As I have specifically pointed out (Tart 1982), this is a tricky process and must not degenerate into ad hominem attacks.

As one example of what I regard as illegitimate use of ad hominem tactics that could mislead readers of this journal, Alcock first establishes me as a central figure in parapsychology ("a former president of the Parapsychological Association") and then reports that I "posit" (a rather abstract, philosophical term) that there is a "widespread" semiconscious or "unconscious fear of psi" that affects our attitudes toward it. He then dismisses this as "too weak and *ad hoc* to require rebuttal." This paints a picture of parapsychologists themselves, as making up unsubstantiated ad hominem excuses instead of dealing directly with issues of nonrobust replicability. What Alcock fails to indicate is that the publications he cites reported a variety of empirical observations about the effects of attitudes, from which I then constructed empirically testable theoretical formulations (e.g., Tart 1982). I believe that making empirical observations and formulating empirically testable hypotheses (not "positing") is the essence of the scientific process, and I have gone on to other empirical studies of the issue (Tart 1986b; Tart & LaBore 1986) which Alcock does not mention. Nor does he mention that I strongly admonish parapsychologists to study their own attitudes as potential sources of bias in experiments, rather than just focusing on critics.

Are scientists disinterested observers? Now let us turn to what I consider the primary thrusts of Alcock's review. Alcock indicates that (1) parapsychologists are not really totally objective and disinterested scientists investigating trivial anomalies but human beings with theoretical preferences and a belief that their work is important, and (2) some parapsychologists may actually be searching for proof of the existence of a human soul! These two observations are used as a basis for discrediting parapsychology as a scientific enterprise.

I do not believe that Alcock's first observation can mean much to any working scientist. Having theoretical preferences and emotional attachments to them is the norm, not the exception. Good science is a matter of admitting them to yourself so you can exercise discipline and give empirical observations priority over your preferences. Related ideas of Alcock's, such as the one that only disinterested scientists should investigate parapsychological phenomena, are unrealistic. You do not devote yourself to what you are not interested in. As R & P's review shows, parapsychologists have devoted far more effort than have researchers in most other fields to make sure their preferences and biases do not introduce artifacts into their experiments.

Are some parapsychologists searching for a soul? The main thrust of Alcock's criticism, however, is that parapsychology is not a science but a search for the soul. This opposition apparently makes parapsychology scientifically illegitimate a priori. Alcock is partly right. Some parapsychologists are interested in the idea of a soul. I find the idea interesting, even important if it represents some sort of reality. But so what? Why is an interest in the idea of a soul inherently unscientific?

I have always taken a common view that science (Tart 1972) is a matter of formulating, extending, and testing explanations about the universe under the guidance of a primary rule that *observations have first priority.* If an observation does not fit your theory, too bad for the theory. Its limitations and perhaps fundamental incorrectness must be recognized. As a psychologist, I have repeatedly observed how scientists, as humans, become emotionally attached to clever theories and how they can become blind to contradictory facts. Alcock emphasizes the enormous improbability of psi and souls, given our knowledge of the physical universe. Can we be sure enough of our current knowledge of the way the universe is constructed to confidently discredit or discourage research that seems to contradict our theories about how the universe runs?

Alcock is inconsistent here. He admonishes parapsychologists to abandon any theoretical ideas about psi and reduce parapsychology to the disinterested, empirical study of anomalies, yet it seems to be his certainty about, and attachment to, current physical theories that stimulates his attack on parapsychology in the first place. Nor did I see Alcock urging more research support for parapsychology so we can really clarify the issues, which would follow if we were purely empirical in our approach.

Why we should study psi. At the beginning of this commentary, I asked why Alcock and other critics make so much fuss about the quite small-scale activity of parapsychology. I agree with Alcock's stated rationale: Insofar as psi represents real phenomena (and I think empirical evidence shows it does), the implications of psi are important and perhaps revolutionary.

To illustrate briefly: Contemporary scientific knowledge of the physical universe is immensely useful and needs to be extended as much as possible. As a result of years of psychological observation, however, I have found that the confused amalgamation of empirical knowledge with a philosophical position of materialism has resulted in a dogmatic "religion" of *scientism* that is destructive of sources of value and vitality to humans (Tart 1986a). The opposite belief, that we may have some sort of soul and that there are spiritual realities (ignoring the frequent pathological uses of these ideas), *can* be revitalizing to people. The effects of such beliefs are legitimate subject matter for psychology. Even more fundamentally, such beliefs can be formulated as testable theories: Do we have empirical data that make more sense in terms of a "soul theory" or psi theory than in terms of other points of view (see Tart 1981)? Are psychological beliefs about souls based on important facts? As a scientist, if I can contribute any empirical data bearing on the question of the reality of the soul, I believe I am doing something useful, not something suspect and a priori unscientific. One of the few things I can fault in Rao & Palmer's review is too little attention to the humanly vital implications of parapsychological studies.

I have too much confidence in the basic long-term usefulness of scientific methodology and too much commitment to the principle of genuinely free inquiry to want to see the current corpus of scientific knowledge treated as if it were some kind of religious orthodoxy that needed defense against heresy. Yes, the implications of psi may be startling, but if you view science as an adventure and as a quest for truth, as I do, that is the reason to work with it, not to reject it out of hand without examination!

The psi controversy as a crystallization of the conflict between the mechanistic and the transcendental worldviews

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A continuing controversy centers on empirical evidence for the existence of certain purported "paranormal" phenomena, especially such psi processes as clairvoyance, precognition, telepathy, and psychokinesis (PK). Paranormal phenomena are those which, if valid, would violate a "basic limiting principle" of science (Broad 1953). Although parapsychology is generally defined as the scientific study of psi, some parapsychologists study spiritualistic, occult, and other ostensibly paranormal phenomena as well.

In analyzing this controversy, one must distinguish two separate issues: (1) Is there scientifically acceptable evidence (i.e., replicable evidence based on observation or experimentation) for psi? and (2) What are the implications of the reality status of psi for our worldview (i.e., for our models of the person, reality, and knowledge)? Clearly, the controversy concerning the existence of psi is more than an empirical matter. The conflicting conclusions of different scientists examining the same evidence do not attest merely to the complexity and ambiguity of the evidence or the temperamental differences among scientists (e.g., James's [1907/1978] "toughminded vs. tenderminded" characterology). This controversy concerning evidence for the existence of psi reflects a conflict between the meaning of different worldviews. Indeed, the controversy about the reality status of psi may be the current crystalization of some of the historically most significant philosophical issues about the nature of the person, reality, and knowledge.

I will review implications of the reality status of psi for some of these philosophical issues. In so doing, I assume that the validation of psi as paranormal would violate basic limiting principles of science, resulting in a Kuhnian (1962) revolution in science and in the society as a whole. If psi phenomena were validated, but their mechanisms were not paranormal (i.e., they did not violate basic limiting principles of science), then no revolutionary consequences need result. Some of these philosophical issues are:

1. *The mind–body problem.* The tradition of dualism, particularly dualistic interactionism, in which man is thought to possess *both* a body and a mind/spirit/soul, would be supported by the validation of psi. Such dualistic notions are a foundation for our religious belief systems. However, materialistic monism is the "working" model of most theory and research in current science, as well as of certain philosophies of life such as Marxism and secular humanism. The validity of materialistic monism is supported by the enormous progress of humanity through history in scientific and technological development.

2. *Vitalism versus mechanism.* Vitalism – the notion that life possesses qualities that cannot be explained in purely physical and chemical terms – would be supported by the validation of psi. However, the mechanistic notion that life can be fully explained in terms of physical and chemical processes has been an overwhelmingly successful working model in the history of science and technology.

3. *Supernaturalism versus naturalism.* The existence of psi is consistent with supernaturalism: the notion that an adequate explanation of nature requires transcendent principles. Naturalism is a critical assumption for the scientific paradigm, which assumes that nature, including the person, can be adequately explained by principles within it.

4. *The person is separate (i.e., qualitatively different) from nature versus the person is an unexceptional part of nature.* The validation of psi as a feature of human consciousness would imply that the person transcends nature. However, scientific findings, especially those from molecular biology and chemistry, strongly support the notion that the person is a characteristic phenomenon of nature.

5. *Determinism versus indeterminism.* Certain psi phenomena imply that a future event, before it occurs, can directly affect a present event. Such a model is inconsistent with the deterministic model of causality that underlies scientific and mundane knowledge (i.e., an event is completely explicable in terms of its antecedents).

6. *Survival of physical death by human consciousness versus nonsurvival.* The validation of certain psi phenomena (e.g., remote viewing, spiritualistic phenomena) would indirectly support the possibility of survival after physical death. This notion is fundamental to the magical/religious belief systems of all known cultures and may be psychologically the most potent wish of humanity, both individually and collectively (Becker 1973; Rank 1950).

One integrative theme penetrating these six issues may be the following: Human consciousness possesses transcendental qualities (transcendental worldview) versus human consciousness reflects only mechanistic qualities (mechanistic worldview). Thus, the validation of psi as paranormal would conflict with the most fundamental philosophical foundation of science, the mechanistic worldview. This mechanistic worldview provides the matrix for the organization and interpretation of the mundane and scientific knowledge accumulated throughout history. Strong evidence and compelling urgency (indicated by a crisis in the current scientific paradigm) would appear necessary to warrant a profound reorganization in this worldview.

The validation of psi as paranormal would be consistent with the transcendental worldview and would thus support some of the most potent human wishes/beliefs/values. History shows that we must be especially vigilant in evaluating evidence relevant to belief systems that confer "special status" on humans, especially when that special status is congruent with potent, perhaps universal, wishes/beliefs/values. As recounted by Brandon (1983), the history of physical research (scientific investigation of spiritualism) appears especially replete with fraud, erroneous observations, and conclusions confounded by the wishes/expectations/beliefs of the participants. There are still ongoing controversies about scientific systems that "dethroned" humanity from special status by conflicting with cherished beliefs (e.g., Darwin's Evolutionary Theory, Freud's Psychoanalysis). [See *BBS* multiple book reviews of Kitcher's

Vaulting Ambition BBS 10 (1) 1987 and Grünbaums *The Foundations of Psychoanalysis BBS* 9(2) 1986.]

I will construct a "truth table" to speculate about the potential sociocultural consequences of each of the four possible outcomes of the two different beliefs (psi exists; psi does not exist) with the two possible states of reality (psi exists; psi does not exist).

1. *Belief: Psi exists. Reality: Psi exists. Correct decision.* This would be a correct belief in the transcendental worldview. The basic limiting principles of science would be modified or rejected. A Kuhnian (1962) scientific revolution would transform our models of the person, reality, and knowledge. Such a new worldview might imply vast, heretofore unknown, potential for humanity. Some of the most potent wishes/beliefs/values of humanity might be empirically validated.

2. *Belief: Psi exists. Reality: Psi does not exist. Type I error.* This would be an incorrect belief in the transcendental worldview, resulting in incorrect models of the person, reality, and knowledge. Such an incorrect worldview might be deleterious for cultural and scientific development by cultivating belief systems with authoritarian/intuitive/mystical origins that institutionalize incorrect assumptions about the person, reality, and knowledge. Many human resources might be misapplied and wasted. Persons might seek meaning within belief systems that are false.

3. *Belief: Psi does not exist. Reality: Psi does not exist. Correct belief.* This would be a correct belief in the mechanistic worldview. Belief systems based on rational/empirical evidence might replace those based on authority, intuition, and mysticism. Humans might implement naturalistic philosophies of life, such as Marxism or secular humanism, which might replace supernaturalistic belief systems. Scientifically generated value systems might become a reality.

4. *Belief: Psi does not exist. Reality: Psi does exist. Type II error.* This would be an incorrect belief in the mechanistic worldview. Anomalies would be erroneously explained in terms of known mechanistic principles. Many potentialities would be unknown, unaccepted, and unactualized, resulting in a waste of human potential and in limitations in our models of the person, reality, and knowledge.

To summarize: (1) Currently there is ambiguity in the meaning of empirical findings concerning the reality of psi; more and better designed research is needed. (2) The need for more research is especially compelling when one considers the potentially serious consequences of a Type I or a Type II error. (3) Verification of psi as paranormal is congruent with many of the most potent wishes/desires/values of humanity and, thus, research concerning this issue requires sophisticated controls and great caution. (4) Verification of psi as paranormal would result in a scientific revolution because it would require modification or rejection of the mechanistic worldview. This worldview provides the matrix for the organization and interpretation of nearly all of our scientific and mundane knowledge. Thus, strong evidence and compelling urgency would be the minimal requirements for such a profound change in worldview.

I recommend an orientation of "probabilistic skepticism" toward the reality of psi processes (and generally toward all ostensibly paranormal phenomena). According to this orientation, all classes of explanations of ostensibly paranormal phenomena might be considered possible and worthy of consideration. However, the investigation of different classes of explanations might be given priority according to probabilities derived from our scientific knowledge base. Through thousands of years of accrued mundane life experience and hundreds of years of scientific investigation, certain classes of explanation for phenomena are known to be differentially probable. Some mechanisms have been empirically demonstrated so often that their probabilities as potential explanations are relatively high, whereas other mechanisms appear less likely as explanations. The scientific community must remain open-minded and, concurrently, we must use our scientific knowledge base as well as our limited resources optimally, initially emphasizing the investigation of more probable explanations. Then, should these explanations not account satisfactorily for anomalies, greater resources must be expended on the study of less probable explanations. Although a conservative orientation, probabilistic skepticism allows for a relatively open-minded consideration of anomalies.

Anomaly versus artifact, or anomalous artifact?

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Both target articles overstate their arguments and evidence, and there are many specific points made by each with which I disagree. I will not raise such particulars here, however, as I expect that the other commentators will raise them. I confine myself here to some broader issues.

I am less troubled by Rao & Palmer's (R & P's) paper than by Alcock's. R & P take a very conservative parapsychological position, emphasizing the merely anomalous character of much of the psi-supportive data; and they clearly recognize that such data may ultimately find explanation in new sorts of nonpsi artifacts. They recognize, then, that the major contribution of parapsychology may someday be judged to have been more methodological than substantive. They mainly argue that the evidence they present should give us reasonable cause to think something new and interesting is happening. Unlike many of their critics, they recognize that evidence is a matter of degree; it is not a case of whether or not there is *any* evidence for psi. The real question is, Is there *enough* evidence for the heavy burden of proof parapsychologists must bear for such an extraordinary claim as psi? Although R & P find the quality and quantity of such evidence as they present us to be convincing (and they hope we will agree), they recognize (as does the 1986 statement by the Parapsychological Association) that psi remains an hypothesis rather than a validated fact. Because few critics would dispute psi as a mere hypothesis, the real issue then is, What sort of priority for investigation should the psi hypothesis be given by other scientists? Clearly, R & P think the scientific priority others give psi research should be higher than it has been.

Like Alcock, I find that the evidence for psi remains unconvincing; but I think Alcock goes beyond skepticism (doubt and nonbelief) to disbelief and advocacy of the materialism/monism of the dominant (orthodox) psychological outlook. And I think he assumes that that position does not need to bear any burden of proof. Alcock takes issue with the weight of the evidence R & P have offered us, but the assignment of such weight is something negotiable in terms of proper argument and evidence. Looking closely at their papers, I find Alcock and R & P in many ways closer to one another on these matters than I would place Alcock relative to some of his fellow hardline psi critics or R & P relative to some of their more "humanistic" and nonexperimental colleagues. But Alcock insists that most parapsychologists are open or closet dualists, and he argues that there is a fundamental incommensurability between their views and those of the mainline (materialistic/monistic) psychology he represents. Despite R & P's claim that reasonable scientists might have reasonable differences that can yet be adjudicated through increased empirical research, Alcock seems to view dualism as so fundamentally unreasonable that there is little possibility of eventual agreement based on future experiments. And because the psi hypothesis supposedly stems from a motivation to demonstrate dualism, I read Alcock as implying that even the hypothesis is unreasonable.

Clearly, Alcock's attack is less on the data for psi than on the psi researchers' "hidden agenda." The wide diversity of psi researchers' views on such philosophical matters simply contradicts Alcock's charge; clearly, such an agenda is not apparent in the R & P approach, and it is explicitly denied by many parapsychologists I know. (It should also be noted that Alcock neatly ignores the existence of a wide variety of dualistic and monistic philosophies of mind.) In any case, however, Alcock's attack on motivation is here quite irrelevant insofar as the data gathered by the psi researchers is uncontaminated by it. Should we reject Newton because at heart he was an alchemist? I believe that Alcock is really presenting us with a kind of ad hominem argument. Worse, he treats psi researchers who might falisfy his claim as exceptional or unimportant. Thus, he comments that "those in parapsychology who move closer to the skeptical side will fail to draw the rest of parapsychology along with them." If this is true – and I think the evidence is very strong that it is not (*e.g.*, R & P speak for their contemporary constituents) – *so what*? If the others reveal themselves as supernaturalists when the evidence clearly goes against them, let them do so and be clearly labeled "metaphysicians" or "pseudoscientists" by us all. On the other hand, let us not prematurely reject those who now pledge allegiance to scientific method because we suspect that they may later reject it if the data went opposite to their philosophical hopes. We need to remember that those who continued to believe in spirits left parapsychology when it discarded the remnants of its spiritualistic connections. When I speak of a rapprochement between parapsychology and anomalistic psychology (and in the beginning even J. B. Rhine [1949] expected psychology to eventually subsume parapsychology), I assume and expect some hardliners on *both* sides to be unwilling to join in the new emerging consensus.

I think Alcock fails to appreciate that parapsychology emerged not as a return to supernaturalism but as an attempt to naturalize the supernatural. Its history – going from spiritualism to psychical research to laboratory work, first on ESP and then on "mere" anomalies – has been progressive rather than regressive. Unlike much modern psychology that has been either denying or dismissive of alleged "transcendent" experiences, parapsychology has tried to build a bridge from this spiritual domain to mainland science. In doing so, it has been criticized as "unspiritual" by theologians and "unscientific" by materialistic scientists. Psi research may have been initiated as seeking proof for a soul, but emphasis has rapidly shifted to the investigation of anomalies. (Alcock surely would not condemn chemistry because its roots were in alchemy and its search for the Philosopher's Stone?) Granted that from the standpoint of the dominant (materialistic/monistic) contemporary psychology, much psi research may look like a throwback; but Alcock apparently naively presumes an undisputed scientific validity for the Orthodox Psychology he espouses. This assumes a uniformity with mainstream psychology that is easily contested (e.g., it ignores the new dualisms being put forward by several prominent theorists in neuroscience), and it ignores deep methodological problems with what is still our very incomplete ability to explain and predict human thought or behavior.

In the end, critics need to test their theories as legitimate rivals to psi; they can not continue to dismiss results on an *ad hoc* basis as they have too often done using plausible but undemonstrated counterexplanations. I agree with R & P that something interesting and probably new is going on; but I think it may be new kinds of artifacts yet to be discovered. There may yet be undiscovered sources of error, just as there may be new areas of truth. I urge that hypotheses be put forward as to what such proposed artifacts (new and old) might be; but in the end, the answers will most probably come from laboratories and not from armchairs. In the meantime, let us follow the dictum of C. S. Peirce and seek to "do nothing that might block inquiry" (p. 56).

Psi, statistics, and society

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One need not read past the first sentence of the abstracts of Alcock's and Rao & Palmer's (R & P's) target articles to realize that personal opinion plays a major role in one's assessment of the evidence for or against psi. If we accept this as true, then we must also assume that the authors hope to convert us to their positions. Thus, on the one hand, Alcock attempts to convince us that "parapsychology, over its century or so of existence as an empirical research endeavor, has simply failed to produce evidence worthy of scientific status"; on the other hand, R & P claim that "there are good experiments that seem to provide evidence for the existence of psi."

How can the same evidence be viewed in such contradictory ways? As a statistician, I believe the distinction between Type I and Type II errors is relevant. In hypothesis testing, the rejection of a true null hypothesis is called a Type I error, and failure to reject a false null hypothesis is known as a Type II error. When setting the level of significance for a statistical test, researchers must weigh the consequences of each type of error. In relation to psi, the null hypothesis is that no such abilities exist. A Type I error would occur if we were to claim that psi exists when it does not. In contrast, a Type II error would occur if we were to fail to accept the existence of psi abilities that do indeed exist.

Because the acceptance of psi requires the rejection of many assumptions on which Western science is based, most scientists regard a Type I error as more serious than a Type II error. It is therefore not surprising that, as Alcock points out, 50% of surveyed scientists consider ESP to be impossible or a remote possibility. Very few scientists read firsthand reports of psi research, and their "acceptance of the null hypothesis" is rarely based on empirical data. As any astute statistician knows, the best way to accept a null hypothesis is to collect very little data.

From the standpoint of the public, however, a Type II error regarding psi is more serious than a Type I error. This is true for two reasons. First, many scientific breakthroughs that are initially viewed as crazy later come to benefit society. If psi abilities exist and can be learned, there is potential for vast societal change. By prematurely ending psi exploration, humanity might be severely shortchanged.

The second reason for avoiding a Type II error concerns the role of science in society. In a recent survey conducted by the University of Chicago's National Opinion Research Council, 67% of the 1,473 adults surveyed claimed that they had experienced ESP. This figure represents a 9% increase over results from a similar study in 1973 (Greeley 1987a). As Alcock notes, these experiences often have a powerful emotional impact.

Society has increasingly turned to science for answers to complicated questions, and science has gained respectability by providing answers to those questions. In rejecting a phenomenon experienced by two out of every three adults, science is shooting itself in the foot. Funding for scientific research is coupled to public opinion, and public opinion is closely tied to how well science can provide answers to questions concerning everyday experience.

Even so, the differences expressed by the authors of these two articles go beyond assessing the seriousness of Type I and Type II errors. One major difference of opinion focuses on the degree to which significant results must be replicated. As Tversky and

Kahneman (1982, Chap. 2) have shown, most psychologists – and by implication, most other scientists as well – do not understand the concept of sampling variability. Thus, if a study fails to replicate a controversial claim, many researchers assume that the initial claim was false.

When effect sizes are small, however, very few studies should be expected to obtain significant differences from chance. For example, I have shown elsewhere (Utts 1987, p. 396) that a typical ganzfeld study should be expected to obtain significant results only about one third of the time, even if the true hit rate is as high as 38% instead of the hypothesized 25%. Consequently, replications should not necessarily be expected to confirm the original results.

Because p-values are dependent upon sample size, the replication problem can be eased by focusing on the size of an effect as well as on the magnitude of its associated p-value. For example, R & P report that Schmidt's (1969b) first experiment resulted in $p < 2 \times 10^{-9}$. This value makes the result appear exceedingly significant. However, a 95% confidence interval for the true probability of success covers only .2575 to .2644, where chance is .25. Thus, the magnitude of the effect, if there is one, is very small; and a confidence interval allows independent readers to assess whether or not a difference of that magnitude could be due to factors other than psi.

One point on which there seems to be agreement is that if psi abilities do exist, then the effects that have been measured so far are weak. The uniformity of weak effects raises the question of practical significance, as opposed to statistical significance. R & P address this issue by stressing the "basic research" aspect of parapsychology and by recommending the use of information theory to enhance the weak signal.

May and his colleagues (1985) have proposed a theory to account for the weak effects in random number generator studies. This theory, called Intuitive Data Sorting (IDS) hypothesizes that subjects are not able to physically alter the random source, but, rather, they are able to use precognition to push the button when favorable sequences are about to occur. According to the theory of IDS, subjects cannot produce strong effects because they are merely taking advantage of statistical fluctuations in the data.

If this theory proves to be correct, it could have profound practical significance even without replicable "strong effects." Taken beyond the realm of random number generator studies, the theory suggests that IDS could have elicited favorable outcomes in any study that used a human decision to carry out randomization. Because randomization is at the core of most statistical analyses, the confirmation of IDS would throw a new light on much of the research conducted in this century.

In summary, given the fact that nonrepeatability is a feature of sampling variability, especially with small effect sizes, and given the predisposition of most scientists to avoid a Type I error when it comes to parapsychology, the existence of psi is still an open question. The potential consequences of continuing psi research clearly outweigh any empirically derived reasons for its abandonment. Exchanges such as this one in *BBS* can only serve to enhance the quality of the research, so that perhaps the issue can be settled long before the passing of another century.

Distance, ESP, and ideology

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In his target article (sect. 5.2), Alcock refers to the widespread commonplace in parapsychology that the scoring patterns in ESP (extrasensory perception) and PK (psychokinesis) experiments were found to be independent of physical factors such as distance, time, and complexity of the target system (e.g., Rush 1986). J. B. Rhine used this independence in substantiating his suggestion about the nonphysical nature of psi phenomena (Rhine, J. B. 1954; Rhine & Pratt 1962), a suggestion that has long been accepted in mainstream parapsychology (Beloff 1980a; 1983; Rao 1978a; Tart 1977; Winkelman 1980). The skeptics, on the other hand, used the same independence in substantiating their suggestion about the artifactual nature of experimental psi results (Gardner 1977; Hansel 1980; Reber 1982), a suggestion that was corroborated by no lesser authority than Albert Einstein. He wrote in a letter to Ehrenwald (1978): "My impression regarding the card experiments which are amenable to numerical treatment is as follows: On the one hand I have no objection to the reliability of the method. Yet I find it suspicious that 'clairvoyance' [tests] yield the same probabilities as 'telepathy', and that the subject's distance from the target cards, i.e., from the agent, should have no influence upon results. This is improbable to the highest degree and consequently the result is suspicious" (p. 138).

I shall discuss some problems in evaluating the supposed empirical evidence for distance independence of ESP. Considerations regarding time can be found, for example, in Schmidt (1976; 1981a), Braude (1982), and Tart (1983); discussions of the complexity of the target system appear in Kennedy (1978) and Vassy (1985; 1986).

The original reasoning by Rhine and Pratt (1962) can be summarized very simply. First, there were instances of successful ESP experiments with several thousand miles between sender and receiver; second, in experiments done with varying distances, there was no significant relationship between distance and ESP scores.

The first argument is not really relevant, for the following reason. If we know neither a theoretical nor an empirical baseline effect size for short-distance ESP in a given experimental situation, then the mere existence of some qualitatively significant long-distance effect does not say anything about a potential distance *dependence*. For example, by shortwave radio communication, we can send messages to practically any region of the globe; but we still know that the physical vehicle of radio communication does depend on distance. To establish a baseline, we must do experiments with both short and long distances (ensuring that all other factors are equal), and then we must compare them; this is just the kind of experiment mentioned in the second part of the Rhine and Pratt reasoning.

Rhine and Pratt (1962) have not given a systematic analysis of comparisons. Instead, where they had found no qualitative differences between short- and long-distance scores, they interpreted it as an indication of distance independence; and where they had found some differences, they interpreted it as "determined by other conditions, most probably psychological" (Rhine & Pratt 1962, p. 68). Later, a systematic analysis was made by Osis and Turner (1965) of all the experiments published in English up to that time. They found two definite patterns in their data base: (1) The average score as a function of distance was *steadily decreasing* in the whole measured range (100 yd – 8,000 mi); (2) in all (seven) experimental series where the same subject was measured at two different distances, the average score was *higher in the short-distance case*, in four series significantly so at $p < .01$. They defined a variable that characterized the transmitted amount of information and calculated the .95 confidence interval for the power of the distance dependence of that variable; this was $(-.409, -.378)$ for the whole data base. That is, denoting the distance by r, they found the distance dependence of the transmitted amount of information to be roughly $1/(r^{2/5})$.

Kogan (1968) and his coworkers carried out another series of experiments in the Soviet Union to study (among other factors) the distance dependence of ESP. They used a different method. The dependent variable was the time density of the transmitted amount of information; that is, the transmitted amount of information divided by the length of the prespecified transmission period. They did not analyze their results statistically, but they obtained a definite *quantitative decrease* for their variable between a few meters and 4,000 km (about 2500 miles).

Nelson et al. (1986; in more detail, Dunne et al. 1983), with the technique of so-called remote perception, have not found significant distance differences up to > 1,000 miles in a data base of 334 experimental trials. They wrote: "Certainly, there is no $1/r^2$ dependence that might be expected for various wave-propagation mechanisms that have been proposed for such phenomena" (Nelson et al. 1986, p. 281).

To my knowledge, these have been the only studies to date dealing systematically with distance dependence. Their methodological quality does not always meet present-day standards of parapsychology; the Kogan work is especially weak, or at least some important details are missing from the report. Further experimental investigation is needed, and no outcome is yet definitely ruled out.

On the theoretical side, some conceptual problems were recognized even in the early Rhinean period. Hoffmann (1940) pointed out the difference between signal *intensity* and signal *intelligibility*. In short, even if the intensity of a physical signal decreases (within a given range) with distance, the message conveyed by that signal does not necessarily becomes less intelligible. For example, flashlight signals in Morse code can be understood equally well in a large range of intensities. Moreover, the intensity itself does not necessarily decrease with $1/r^2$; as Rush (1943) remarked, "the inverse-square propagation of energy is seldom realized in practice. Such effects as diffraction, reflection, refraction, and absorption, as well as deliberate 'beaming' in the case of radio signals, modify the simple spatial distribution" (p. 48). (By the way, this explains the fact I mentioned earlier that radio waves can reach almost any point of the Earth's surface with enough intensity for reception.) Until we know the modus operandi of ESP, we should not expect any specific decrease pattern, and particularly not the inverse-square law, which for all known kinds of interaction applies only to very specific circumstances. More recently, Dobbs (1964) analyzed some theoretical possibilities and empirical findings for distance dependence along similar lines, with the conclusion that any simple wavelike transmission is indeed improbable, but this does not rule out other solutions "within the framework of the physicalist hypothesis" (Dobbs 1964, p. 247).

In the light of these considerations, it is surprising how confidently the "distance independence" argument (together with the other "independence" arguments) is used by both parapsychologists and skeptics. Of course the former side is primarily to blame: The skeptic (or an impartial observer like Einstein) cannot be realistically expected to dig into the literature and recognize enduring theoretical superstitions not recognized by the parapsychologists themselves. In my opinion, this is an example of how the ideological orientation of a research field influences the conceptual framework used in interpreting the existing empirical data base. Rhine and his ideological followers *want* to find independence from physical factors, and they find it; the skeptics *want* to find a general haphazardness and lawlessness, and they find that. If the facts do not fit into the pattern, too bad for the facts.

Science and rationality

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It appears that the views of these contenders, Rao & Palmer (R & P) versus Alcock, are respected in the scientific community. Surely this would not be the case if the issue were the existence of Santa Claus. The airing of this issue in *BBS* provides evidence that the scientific community continues to entertain the possibility of psi despite the fact that there is no integrated body of knowledge in which it fits, its existence conflicts with existent bodies of knowledge, and it has no theoretical basis of its own. Alcock accuses the parapsychologists of having a "general willingness to suspend doubt" whereas R & P counter with the contention that "such a stance could be seen to be sheer dogmatism and the very antithesis of the basic assumption of science's open-endedness." Thus psi may be regarded as an issue somewhere between the study of Santa Claus and the study of, say, the AIDS virus. Where do we (the scientific community) draw the line?

Another major issue centers around the efficacy of investing money and brainpower in psi research. Alcock states that "over its century or so of existence as an empirical research endeavor, [parapsychology] has simply failed to produce evidence worthy of scientific status." R & P counter with the statements that "there have been hundreds of experimental reports of evidence for psi" and "the small magnitude of most current psi effects is irrelevant" (sect. 2, para. 5). Based, in part, on the latter statement, it seems clear that the study of anomalies within the context of physical systems that, relatively speaking, we know and understand, is going to be more productive than the attempt to understand psi in the context of speculative psychical systems that lack continuity and integration. No one (and clearly not Alcock) expects Palmer's (1986a) recommendations in this regard to be implemented by the parapsychologists. Alcock attributes his view to motivation on the part of parapsychologists to open up a rational basis for individual survival or immortality. Thus, according to Alcock (and I agree), parapsychologists are not going to be dissuaded from seeking a rational basis to support irrational beliefs.

This view may be regarded as dogmatic and negativistic, but my comments are not directed at these beliefs or at the believers. Rather, they are directed at those who would expend valuable resources in the name of science to investigate empirically hypotheses that have no rational basis. What this accomplishes is to mislead the general public about the nature of the scientific enterprise.

The question about what science is or should be has been a source of much controversy. However, few would deny that it is a rational enterprise. On the other hand, it is simplistic at best to equate rational with "good" and irrational with "bad," for that would negate, for example, the arts. Investments in irrational causes can be "good" or "bad," but neither way can they be deemed science. As with the issue of creationism, psi phenomena as dealt with by the parapsychologists are not science and are thereby out of our purview.

It seems clear that Alcock does not perceive a rational basis for doing psi research. Furthermore, he has noted that a rational basis has never existed in the century these research efforts have spanned. Similarly, R & P recognize that within the scientific community, disbelievers have prevailed in the past and continue to do so in the present. Some people will continue to do psi research, just as some people will continue to climb mountains or seek the Holy Grail. Science has played a large role in many such irrational enterprises. Although many regard such adventures as ill-advised, there are lots of worse things people could be doing – so leave them alone. It's a free country, and it's none of our business.

Are scientists materialistic monists?

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Since when is "mind–body dualism" a criterion by which to censure a scientific theory? And with what right is "materialistic monism" a *desideratum* of good science? Are not monism and dualism metaphysical positions allowable to any scientific claim? I suspect we have a category mistake here.

A glance at the history of psychology reveals a continuing discussion of parapsychological phenomena. This discussion does not align with any particular metaphysical position. In fact, an ironical shift has occurred in the metaphysical assumptions of this discourse. In past times, spiritualists were both monists and pluralists; now we have the materialists à la Alcock contending for the honor of monism.

In the nineteenth century, leading friends of the paranormal included G. T. Fechner and William James. Both have been called spiritualists. Fechner was a monist and James a pluralist; both shared a conviction in the reality of the spiritual (James 1909; 1984). In this instance, spiritualism was on the side of nineteenth-century science (Daston 1982).

On the other side of the scientific ledger stood Hermann Lotze (Woodward 1975; Woodward & Pester 1987), Wilhelm Wundt (Mischel 1970), and Sigmund Freud (Parisi 1987), who were mind–body dualists at the phenomenal level with their occasionalism and psychophysical parallelism. They believed that, methodologically speaking, the body was "occasioning cause" or "physiological substrate" to consciousness, which operated by laws of a different order from the physical. Did they believe in the paranormal? No. As in the case of critics of the paranormal today, perhaps the rigor of their standards of evidence and psychological laws precluded the acceptance of psychic phenomena. Yet in spite of their related but distinct methodological allegiances to identity, interaction, and parallelism, Fechner, Lotze, and Wundt shared an ontological conviction (with Leibniz) that the world was a spiritual monad and humanity a microcosm thereof (Höffding 1900, pp. 508–31; Paulsen 1907, pp. 99–103; 128–30).

Does Alcock still contend that materialism is the way of science, and dualism the refuge of parapsychology? If so, let him take solace in the fact that Fechner (the nineteenth-century spiritualist) is newly interpreted in twentieth-century terms as a materialist (Heidelberger, in press a) and an indeterminist (Heidelberger, in press b). But let the rest of us, parascientists or not, enjoy the right to our own metaphysical commitments.

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Author's Response

Where lies the bias?

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We welcome this opportunity to respond to the commentaries on our target article. We are pleased with the thoughtfulness, sophistication, and sobriety of most of these commentaries, and we particularly welcome the contributions of many who do not ordinarily participate in such debates. We share the view that it is time for parapsychologists and their critics "to play in the same ballgame" and observe the same rules, so that an empirical convergence of opinion can be reached. We have tried our best to respond to most of our critics' points, although a few of the "minor" ones remain unaddressed, and there are some we would have liked to elaborate upon further if we had had unlimited space. We have organized our response under the following three headings: Semantic Issues, Methodological Issues, and Theoretical/ Philosophical Issues.

1. Semantic issues

We attempted in our target article to redefine the field of parapsychology and to use concepts in a way that would provide for all the participants in this debate a common universe of discourse. If we as parapsychologists are somewhat untraditional in this, we are neither driven by a desire to look "respectable" to follow scientists nor drawn by a wish "to placate the critic." Our motivation is simple and straightforward, that is, to lay a common ground for a meaningful exchange of views between parapsychologists and their critics. We were apparently not entirely successful, because a few of our commentators (most notably **Akers** and **Railton**) have seen some ambiguities. Certain clarifications may accordingly be in order.

1.1. Anomalies. We pointed out in our target article that psi anomalies are the subject matter of parapsychology. An anomaly is perhaps akin to what **Railton** labels a "hec phenomenon." There are obviously many anomalous phenomena in the discipline of psychology, but only those that seem to go beyond the known limitations of our sensorimotor system to interact with, influence, and be influenced by external systems are properly called *psi* anomalies. To qualify as a psi anomaly, then, a phenomenon should (a) arise from interactions between living organisms and their environment, (b) appear to deviate from commonly accepted notions regarding the limitations of sensory/motor capacities, and (c) remain unexplained.

Statements such as (a) and (b) above are not intended to prejudge the cause or origin of the anomalies, but simply to specify the particular subclass of anomalies that interest us – "anomalous communication processes," as some of our colleagues might label them. Thus we would not agree with **Railton** that even his psi-1 should be defined so broadly as to include any scientific anomaly that happens to involve a psychological variable.

A claimed instance of telepathic communication between a mother and her child, for example, would be a psi anomaly if no conventional channel of communication appeared to be responsible for the exchange of information between them and if the mode of communication remained essentially unexplained. It would cease to be an anomaly if it were shown, for instance, that the communication was likely to have been mediated through subtle sensory cues – a conventional explanation. It would also cease to be an anomaly if it were established that the telepathic message was carried, let us say, by extralong electromagnetic waves excited by biocurrents – a nonconventional (omegic) explanation because the brain is not known to generate extralong waves that could carry a message. It would likewise cease to be an anomaly if it were to become established that the consciousness/mind under certain conditions becomes unconstrained by space and shows the capacity to directly influence external events, including the consciousness of others.

While arguing that psi anomalies exist, we hoped to persuade our readers that there is empirical evidence that cannot be explained away in any reasonable manner by the conventional hypotheses so far advanced by the critics of parapsychological research. At the same time, we have not closed the door on the possibility that a conventional explanation may be found later for these anomalies, however unlikely it may seem to us at this time. This state of affairs will continue until we find a reasonable explanation, at which time psi would cease to be an anomaly. If an omegic explanation were to become established, we would be dealing in parapsychology with psi, but no longer with psi anomalies. If a conventional explanation were to prevail, we would then no longer call the phenomena psi, although they might still provide a fruitful topic for research. (For further discussion see Palmer, in press.)

Railton's distinctions between psi–1, psi–2, and psi–3 point to the levels of theoretical commitment one is willing to make, whereas we are advocating a psi concept that is theory-neutral to the extent possible and that refers to the phenomena as distinct from their possible explanation or cause (Palmer 1986c; 1987b). We are opposed to any a priori theoretical loading on psi (parapsychological phenomena) in our present state of profound ignorance about its nature. We consider what Railton calls psi–*1* not to be psi at all; at best, it might be described as ostensible psi. Using his terminology, we would have to agree that our claim is psi–2, but we would insist that this is in fact a more neutral or noncommittal claim than psi–1. This point is a central theme of our whole reply.

Parker suggests that the term anomaly might taint us with the brush of less reputable enterprises. We presume that he means fields such as ufology ("ufo" = unidentified flying object) and cryptozoology, which tend to be united with parapsychology under the heading of anomalistics by writers such as **Truzzi.** There certainly is a great deal of nonsense that can be subsumed under such topic headings, but the same can be said of parapsychology. Also, these other anomalistic fields do produce a small but growing amount of good research, such as the carefully collected "astrobiological" data of Gauquelin (e.g., Gauquelin & Gauquelin 1978). Moreover, the term anomaly, if properly understood, also forges a link with the many anomalies that occur within respected scientific disciplines. We all understandably bristle when pop-conventionalists (conventionalists is our term for "skeptics"; cf. Palmer 1986a) bandy about phrases such as "ESP, pyramid power, and the Bermuda Triangle," but we should not let such behavior prevent us from adopting terminology that helps us define our claims more properly. Moreover, our core term remains psi, which distinguishes our subject matter from that of other anomalistic fields.

In conclusion, we see our use of the term anomaly as an important clarification intended to address the complaints by many conventionalists that we overinterpret our data. There is a big difference between claiming that psi phenomena require omegic explanations and claiming that they are unexplained.

1.2. Omega. Our use of omega as a substitute for paranormal is objected to by **Stanford.** We admit to not being conversant with how terms are used in spiritual and occult disciplines, and we intended no such metaphysical connotations for omega. We still think, however, that omega is an improvement over paranormal for the reasons outlined in our target article.

Stanford also objects to defining "paranormality," however labeled, in terms of Broad's (1953) so-called Basic Limiting Principles (BLPs); he implicitly suggests that the term "unconventional" replace "paranormal." We sympathize with much of his reasoning, but we find the term "unconventional" too broad and vague. The BLPs (or something like them) are required to define the particular ways in which "parapsychological" (i.e., nonartifactual) theories are unconventional. Although we strongly agree that the BLPs are not sacrosanct (and, for that reason, probably mislabeled), many other scientists believe they are sacrosanct and use them to declare parapsychological theories unscientific, or requiring unusually strict standards of evidence. Thus, whether or not a theory violates the BLPs is important in a pragmatic sense. Our distinctions must reflect reality, whether we like that reality or not.

2. Methodological issues

Many of the negative comments on psi research are directed toward methodological issues. Yet none of the commentaries on our target article brought to our attention any crippling design defects, methodological flaws, or statistical fallacies in the research on which we based our conclusions. We accordingly see no reason for revising our claim that strong evidence exists in support of psi anomalies. We have dealt with many of the specific methodological criticisms in the target article itself. We shall briefly respond in this section to the major arguments of our commentators against experimental studies of psi that have methodological relevance. But first we must address some more general issues.

2.1. Motives of parapsychologists. The accompanying target article by **Alcock** predictably generated much discussion about the underlying motives of parapsychologists. Are we all motivated by a need to confirm the existence of a soul (secular or otherwise), or to validate metaphysical dualism? These concerns are obviously important to some of us (e.g., **Beloff, Tart**), but so what? Such interests are not necessarily incompatible with due regard for scientific methods and values (unless one adopts the dubious premise that science is necessarily materialistic), and as **Donderi, Spanos & de Groot, Truzzi,** and **Woodward** observe, they provide no basis for rejecting a scientist's research contributions.

In any case, such motivations clearly cannot be attributed to parapsychologists across the board. As noted by **Donderi, Stanford,** and **Truzzi,** our motives and orientations to parapsychology are quite diverse. Nowadays, most leading parapsychologists do not approach psi from a metaphysical perspective. Even **Mackenzie,** who bemoans this fact, nonetheless acknowledges it. We were gratified by how many commentators, including those like **Akers, Hövelmann,** and **Truzzi,** who could hardly be characterized as "true believers," challenged **Alcock**'s claims on this point.

However, it would be equally misleading to claim that all, or even most of us have no concern for the potential importance of the phenomena we are investigating. No one would put up with the frustrations of being a parapsychologist as long as many of us have without believing that there is a reasonable chance that continued psi research will yield important new scientific principles. Moreover, our research literature clearly shows that most of us accept omega as a "working premise" in our so-called process-oriented research. On the other hand, as illustrated by the position paper (Parapsychological Association 1986) cited by **Krippner**, and in contrast to **Bunge**, we are becoming increasingly aware that what we have so far established is a set of provocative anomalies, the nature of which is at present unclear. **Hyman** is right, although not perhaps in quite the way he intended, that we are "hedging our bets." The crucial point is that we *are* open to conventional (including artifactual) explanations of the anomalies, provided they meet generally accepted standards of scientific evidence.

What we object to in **Alcock**'s thesis is the implication that (a) our motives and values are not scientific and that (b) our allegedly strong allegiances to metaphysical beliefs prevent us from conducting research that others should trust. Of course, we have theoretical preferences, and sometimes these may bias our interpretations of data. But as several commentators (**Benassi, Donderi, Gergen, Spanos & de Groot, Tart**) pointed out, such biases are endemic in science generally and indeed in any intellectual endeavor. Some critics of parapsychology seem to have difficulty acknowledging this. We found it remarkable, for example, that **Bauslaugh** writes as if totally oblivious to the fact that the strong biases he attributes to parapsychologists could be applied with equal justification to their critics. In any case, we don't think that parapsychologists' biases are (in most cases) nearly as debilitating as Bauslaugh suggests.

When we stated that "experimental parapsychology grew out of a need to account for people's experiences in the 'real world'" (which is true), we never meant to imply (as **Alcock** suggests) that early parapsychologists did not approach these experiences with particular theoretical (even metaphysical) interests and orientations. It also seems trivially true that to study psi in the laboratory for any purpose, one must generate it first. Determining whether or not psi requires omegic explanations has, of course, been the objective of much psi research. We see absolutely nothing wrong or unscientific about this.

2.2. Antecedent probabilities. The suggestion is made by **Beyerstein, Feder, Flew, Glymour, Navon,** and **Railton** that the antecedent probabilities of various potential explanations of psi should either dictate the acceptability of these explanations or determine the standards by which empirical evidence on their behalf should be judged. Specifically, because the antecedent probability of conventional explanations of psi is considered much greater than that of omegic ones, the latter should be rejected in the absence of "extraordinary" evidence to the contrary.

First, we reject **Railton**'s claim that the principle involved is not a priori. At issue is not the indisputable and impressive success of conventional science in explaining much of nature, including many (but by no means all) of its anomalies, but rather a certain application of the a priori principle of induction (or generalization), namely, the use of science's track record as a basis for inferring the universality of certain laws and theories of nature and/or Broad's (1953) Basic Limiting Principles. Obviously, science requires induction to some degree – one need not empirically reconfirm the law of gravitation every time one drops one's pencil – but these are cases where there are no empirical indications that the laws do not apply. If one's pencil were to levitate, however, we would be faced with an entirely different situation. Here we feel that the application of induction is suspect, and caution dictates an empirical approach.

But our commentators would be willing to accept empirical evidence for omegic explanations – if it were only good enough, that is, "extraordinary." Two things trouble us about this superficially reasonable compromise. First, it is defined in a vague and arbitrary manner that allows it to be operationalized so as to keep one step ahead of the available evidence, whatever that may be. More important, we think the whole notion of having different standards of evidence for different knowledge claims is suspect. As Palmer (1987a) has argued at greater length elsewhere, the application of this rule virtually guarantees (even assuming equal research competence) that the amount of "valid" data supporting a conventional theory will be much greater than the amount of "valid" data supporting its competitors (thereby creating an artifactual bias favoring the former's acceptance), and that this inequity will then be used to justify the induction rule that contributed to the creation of the inequity in the first place. In other words, the logic for applying antecedent probabilities is (to some degree, at least) circular.

We believe that the same standards of evidence should be applied to all scientific knowledge claims. Science has sound methods for rejecting bad evidence – methods that do not require using antecedent probabilities. If people were to report to us anomalous footprints (**Feder**) or "burning snow" (**Navon**), we would not jump to the conclusion that something "paranormal" was happening, but neither would we jump to the conclusion that it was not. We would (as we hope any other scientists would) attempt to objectively and dispassionately judge the merits of the arguments and evidence presented by the claimants and their critics, and we would endeavor to suspend judgment until further evidence could be gathered.

Sometimes, defenders of antecedent probabilities rely on manifestly silly examples – seeing Santa Claus seems to be the favorite – as a reductio ad absurdum. The problem with this argument is that sane adults never seriously report such things, and even if they did, it is highly probable that the claim could be refuted on empirical grounds alone, such as consensuality. Finally, we would add that the ability to suppress emotional reactions such as the "giggle response" and the "boggle response" in evaluating evidence strikes us as a crucial component of intellectual self-discipline.

2.3. Winkelman citation. Although Winkelman (1982) indeed proposed that psi might be involved in certain magical practices of primitive societies, he never suggested, as clearly implied by **Feder**, that "when a shaman says the spirit of the raven has entered his body, that is exactly what has happened." Because the implication that Winkelman subscribes to such a view could subject him to ridicule, we would urge more circumspection in making such attributions.

2.4. Hansel's fraud scenarios. In reply to **Sanders**, we note that it is **Hansel** (1980, Chap. 3), a prominent critic of parapsychology, who has traditionally demanded a foolproof demonstration of psi. Although at times, as in his commentary, he has indicated that repetition on demand would serve the same purpose, such a position is logically incompatible with his primary thesis, because common deceit or error would still be a more probable explanation than omega, using the criteria set forth in his chapter. (Ironically, *we* would not accept replication on demand as proof of omega, because the replication of an effect says nothing about its cause.)

On to **Hansel**'s specific points. Coover's results are in fact highly significant, if analyzed fairly (Thouless 1935; see also Coover 1939). Hansel's criticisms of the Pearce–Pratt experiment have been addressed elsewhere (e.g., Rao 1981c; Stevenson 1967) Although we do not claim that subject fraud was *absolutely* ruled out in this experiment, we stand by our statement that it was better controlled than were earlier, more exploratory efforts. (We did not intend to include the Pratt–Woodruff experiment in this category.) Schmidt (personal communication) informs us that his paper-tape printouts were automatically read by a tape reader and analyzed by computer, contrary to Hansel's suggestion. Hansel's other responses to our points about Schmidt's research, to the extent we understand them, seem largely repetitions of his original criticisms, which we addressed at some length in our target article.

We did not cite **Hansel**'s critique of Ashton et al.'s (1981) ganzfeld experiment because we had already conceded that experimenter fraud cannot be ruled out in any experiment. In this case, the four authors exchanged roles as experimenter, agent, and subject, and Hansel provided various scenarios as to how persons in the first two roles could have cheated. It would almost be more plausible and parsimonious simply to suggest that the four authors made the whole thing up.

Finally, we might mention that the critic (uncited by **Hansel**) who had relatively positive things to say about Schmidt's research was Hyman (1981), who thinks that the conventionalist cause is not well served by Hansel's unsubstantiated speculations about researchers' dishonesty.

2.5. Flaws and interpretations. Several of the commentators, most notably **Alcock, Hyman, Nadon & Kihlstrom**, and **Spanos & de Groot**, argue or imply that psi research does not meet even conventional scientific standards of methodology. To make their case, they cite the numerous flaws (excluding experimenter fraud) that seem to pervade this research.

Flaws logically imply interpretations (either directly or indirectly), and the two cannot be separated in the way **Alcock, Beloff**, and **Hyman** suggest. For Alcock to say, as he does at the end of his commentary, that "all one needs to presume to *account for* the data . . . is that many psi experiments are not as tightly controlled as is claimed" and then to say this is "not . . . meant to be an explanation" strikes us as a blatant contradiction (our italics).[1]

If the interpretations implied by certain flaws are implausible, irrelevant, or highly unlikely as explanations of the data, it is fair to label the flaws as trivial. If such considerations are to be ignored in scientific criticisms (as they frequently are by critics of parapsychology), what would prevent us from condemning as worthless any psychology experiment that fails to control for omega? Psi data – whether one calls them "anomalies" or "ostensible anomalies" – exist, and even **Hyman** admits he does not know how to explain them adequately. Because that is all we mean by "anomaly," he has conceded our main point. We agree that psi data are ambiguous and that attempts to definitively interpret them retrospectively are likely to be futile. It is this very ambiguity that we find challenging and that justifies further research. Our point is simply this: The conventional interpretations so far proposed (or implied) are scientifically inadequate as explanations of the critical mass of the data, and therefore one cannot reasonably assume that the unknown explanations, whatever they may be, are conventional.

We also agree with **Blackmore, Parker**, and **Railton** that the methodological rigor of psi research compares favorably to that of conventional psychology. **Nadon & Kihlstrom** indirectly and unwittingly reinforce this point when they use a discredited hypnotic regression experiment by True (1949) to illustrate how sensory leakage might contaminate ESP experiments. It is clear even from the skimpy report of this study that appeared in *Science* – which did *not* include the incriminating evidence subsequently uncovered – that the experimenter was in full view of the subject and spoke to him extensively. Compare the opportunities for sensory leakage evident in this study with those afforded to subjects in the quite pedestrian ESP–hypnosis study of Casler condemned by Akers (see **Palmer**'s commentary) where (a) the subject and agent were in different rooms, (b) the protocol precluded the agent's talking to the subject, and (c) steps were taken to preclude even incidental auditory cueing. If True's report had been submitted to a mainstream parapsychology journal rather than to *Science*, it would never have been sent out for review, let alone published. Yet hypnosis researchers apparently took True's study seriously for quite some time.

When **Spanos & de Groot** dismiss the entire parapsychological research literature as comprising "badly done" experiments, they draw heavily on the critiques of **Hyman** and **Akers**. Akers acknowledges in his own commentary that the artifacts he attributes to psi experiments are present in other areas of the social sciences, and Hyman concedes that his criteria are more "conservative" than Akers's. We seriously doubt that even "well done" psychology experiments would fare very well under their criteria. For example, in one of Spanos's hypnotic amnesia experiments (Spanos et al. 1984), we note that the method of randomizing the word list to be memorized was not specified (a flaw according to Hyman 1985b), and an experimenter with apparent knowledge of the word list, who also either knew the hypothesis or could easily infer it, was in the room with the subject and might unintentionally have provided verbal or auditory cues to facilitate recall in those conditions where it was expected.

(These remarks are meant as criticisms of Spanos's criticisms, *not* his research.)

As for **Spanos & de Groot**'s other examples: (a) We would like to know why 4 million control trials (cf. Schmidt 1970a) are not a series "of adequate length for a random number generator." (b) The remote viewing research critiqued by Marks and Scott (1986) was never published in a refereed parapsychology journal. (c) The problem of see-through ESP cards was rectified in the 1930s.

When we say that the conventional interpretations of psi experiments are often inadequate and the flaws are trivial, we are not trying to condone suboptimal methodology. Sometimes the flaws are not trivial, and even improbable interpretations are possible and should be eliminated at the design stage for that reason alone. We thus believe that although parapsychological research methods are much better than is acknowledged by our critics, they can and should be improved further. For example, **Hyman**'s criticisms of the ganzfeld research have been accepted by parapsychologists (cf. Hyman & Honorton 1986) and are already being incorporated. Palmer is preparing guidelines for journal referees and authors that should improve documentation of research and analysis procedures in experimental reports. Marcello Truzzi is assisting us in contacting ethical conjurors who can help us avoid being duped by fake psychics.

2.6. Falsifiability. We generally subscribe to the view of falsifiability in science outlined by **Spanos & de Groot** and **Gergen.** Although Schmidt's research, for example, is customarily labeled by its proponents as providing evidence for psi, it could be labeled with equal justification as refuting certain expectations of conventional scientific theory. Looked at thus, the attempts by conventionalists to "salvage" conventional theory by proposing (under the guise of flaws) *ad hoc* counterexplanations of Schmidt's results nicely illustrate the commentators' points.

A more incisive illustration is provided by **Nadon & Kihlstrom**'s critique of Schechter's (1984) meta-analysis of hypnosis–ESP experiments. Schechter tested and failed to confirm specific predictions derived from the hypothesis that the relationship between these two variables is attributable to methodological flaws, a classic example of the hypothetico–deductive method routinely used in science. Obviously, it takes only one flaw to produce an artifactual outcome, but isn't it logical to hypothesize that the more flaws there are, the greater the probability that at least one of them was operative? All of us, including Schechter, agree that one set of refutations is not sufficient to overturn a hypothesis, but Nadon & Kihlstrom refuse even to acknowledge its relevance.

We also find **Nadon & Kihlstrom**'s characterization of Schechter's paper as a "vigorous attempt to rescue a methodologically deficient literature" misleading. It is evident, especially in the final section of Schechter's paper, that he has adopted a very cautious approach and is aware of the limitations of his analysis. Neither Schechter nor Rao & Palmer claimed confirmation of the hypnotic facilitation of psi in the strong sense Nadon & Kihlstrom imply. As scientists, we simply insist that any explanation of the operationally defined hypnosis–ESP relationship, including the artifact hypothesis, be subjected to empirical test.

Finally, we are aware of no evidence to support **Gardner**'s contention that psi scoring covaries reliably with the level of experimental control, either as applied to the Stepanek research (Pratt 1973) or to psi research generally.

2.7. The Big Stuff. We are enjoined by **Braude** and **Glymour** to study the "Big Stuff", of which macro-PK is the major, but not the only, example. We agree with Braude that the Home and Palladino cases have never been adequately explained away (cf. Braude 1986). If the molecular deformations of metal cited by **Eysenck** are in fact nonreproducible by conventional means, this evidence is impressive as well. So why don't we all jump on the bandwagon? One need look no further than the intimidating last paragraph of **Gardner**'s commentary to understand one reason why some of us shy away from such research. More generally, the problem is sensationalism. Because macro-PK is so weird, it attracts disproportionate attention from the popular media, and thus also from publicity-seeking magicians out to fool the public (including scientists) or to discredit their fellow publicity-seeking magicians. The climate thus created makes serious research difficult and tends to destroy its credibility whether the reasons are justified or not. For example, the fact that Crussard's (Crussard & Bouvaist 1978) metal-bending subject admits possessing conjuring skills automatically disqualifies in the eyes of many conventionalists almost any evidence he might produce.

If we really could learn more about psi by studying the Big Stuff rather than the little stuff, as **Braude** suggests, the hassle might be worth it. But we can not agree with Braude. Parapsychology is being held back not by the size of its effects but by their reliability, and reliability is as much a problem in macro-PK as in other areas of parapsychology. Agency in macro-PK is not always as straightforward as Braude implies (cf. Batcheldor 1984), and in any case there are "gifted" subjects who do little stuff. Finally, on the average, smaller effects are easier to control experimentally than larger ones. However, we are by no means opposed to macro-PK research if it is well done.

2.8. The little stuff. The bald assertion by **Glymour** that any "minuscule effect" should be attributed a priori to some unspecified artifact lacks any foundation that we can see and, if followed to its logical conclusion, would invalidate not just parapsychology but particle physics and many other scientific endeavors whose successes speak for themselves. **Navon** escapes the latter objection by making his argument in terms of sample size, but he cites no evidence in support of his implicit conclusion that the null hypothesis can always be rejected if N is as large as it is in parapsychological applications. In REG (random-event generator) research (where the criticism would seem most applicable), there are strong theoretical reasons to expect the null hypothesis to hold, and it does routinely hold in control tests comprising millions of trials. Even if it did not, there would be no valid reason to conclude a priori that the cause is artifactual, especially when the "bias" covaries with a subject's intent.

2.9. Randomness and REGs. We are chided by **Alcock** for taking **Schmidt** at his word about doing adequate control checks of his REG. We were perhaps remiss in not citing an unpublished document in which Schmidt (1969c) described his randomization checks more thoroughly than in his published report. In particular, he refers to checks made of 100-trial blocks taken from those 1,000-trial blocks that exhibited extreme scores.

Gilmore is certainly right that perfect randomness cannot be assumed for any REG. What is required, however, is not perfect randomness, but the elimination of biases, the nature and magnitude of which are sufficient to account for the experimental effects observed. It is not clear to us how Gilmore's method of analysis would be more sensitive to such biases than those reported by Schmidt. Again, it is important to keep in mind that the effects Schmidt observed in his experimental conditions covary in straightforward ways with the intent of the subject. Nonetheless, we appreciate the value of the kinds of Monte Carlo methods suggested by Gilmore and possibly implied by **Dawes,** whose comments we also found valuable.

We simply do not understand what **Cicchetti** means when he says that the actual REG experimental output should be tested against each subject's hit rate. However, we agree that some sort of analysis of the actual target sequences of Schmidt's experiments might help alleviate concerns that for some reason the REGs behave differently in experimental sessions than they do in control sessions, a result which would itself qualify as anomalous. Schmidt has provided us with the raw data from his 1969 experiments, and we are planning to undertake computer analyses that may shed some light on such issues.

2.10. Statistical replicability. It seems that **Blackmore** is overstating her case when she labels psi results as "unreplicable" (without qualification), a point we think we made in our target article. We were surprised to see **Alcock** and **Hansel** question the appropriateness of statistical replicability, and (in Alcock's case) its operationalization in meta-analysis, for parapsychology. We don't understand their logic. Such an approach is necessary not only in most areas of psychology, but even in particle physics (Hedges 1987). The reasons it is necessary in any field that deals with small effect sizes are explained well by **Utts.**

2.11. Hyman & Honorton quote. It is claimed by both **Spanos & de Groot** and (less pointedly) **Railton** that we quoted Hyman & Honorton (1986) out of context regarding their acknowledgment of an "overall significant effect" in the ganzfeld data base. In both the Hyman & Honorton debate and our target article, a sharp distinction was drawn between whether the aggregate data base reflects a statistically significant departure from chance (statistical replicability) and whether such significance (if it exists) can be attributed to methodological flaws such as sensory leakage. In our target article, these questions are addressed in sections 4.1.2 and 4.2.3, respectively. We think Hyman's views are fairly represented in those two sections combined.

2.12. Replication by "skeptics." There is a serious misrepresentation by **Bauslaugh** of our remarks about replication in section 4.2.4 of our target article. We never said that "investigators who are unsympathetic to psi" should not be involved in replications of psi research. In fact, we said just the opposite. Although we stated that some investigators may have been converted to "belief" in psi by their experiments, we never said that they all become "believers" in this manner. Nor did we ever say that all "disbelievers" have ignored the evidence or are incapable of changing their minds. Finally, we never said that "personal beliefs are irrelevant."

We do think, however, that it is unreasonable to hold any scientific theory or hypothesis hostage to replication by those hostile to it. Would **Spanos & de Groot,** who along with **Hansel** seem to take this line, abandon their model for explaining hypnotic effects if some unfriendly "state" theorists consistently failed to replicate some of their key hypnosis experiments? We doubt it. If omega is ever to achieve wide scientific acceptance, it will come from our understanding the psychological processes involved sufficiently well to define precisely the test conditions that will allow a wide range of investigators (including competent, fair-minded [and courageous] neutrals) to obtain psi effects often enough to satisfy the requirements of statistical replicability. If "skeptics" can get the effects too, so much the better.

2.13. Credibility of reports. We are admonished by **Gardner, Nadon & Kihlstrom,** and **Alcock** not to put too much faith in experimental reports. It is certainly true that even fairly elaborate reports may leave out crucial methodological details. Nonetheless, scientists do routinely take research reports at face value, as Nadon & Kihlstrom illustrate abundantly in the last half of their commentary, and somehow the process works. Do they mean, perhaps, that the mistrust (and attempts to verify the mistrust) should be applied selectively to research findings one doesn't happen to like or find plausible? This admittedly common strategy leads to the same kind of bias we discussed under the heading "Antecedent probabilities" (sect. 2.2, this commentary).

A particularly insidious consequence of this mind set is what we call science-by-gossip. One manifestation of this malady is the media shenanigans rightly condemned by **Benassi.** A subtler manifestation is Diaconis's (1978) casual remark (endorsed by **Nadon & Kihlstrom**) about his subjective impressions of psi research based on visits, under who-knows-what circumstances, to certain unnamed parapsychological laboratories. How are parapsychologists supposed to defend themselves against this kind of innuendo? What can any thoughtful reader conclude from it? The formal reporting mechanisms of science clearly have their drawbacks, but the alternatives we've seen reflect an anarchy that confirms the most unflattering depictions of science by radical sociologists and philosophers such as Feyerabend (1978).

2.14. Cooperation with conventionalists. Cooperation between advocates of competing theoretical viewpoints is always desirable, and the recent Hyman & Honorton (1986) "joint communiqué" received good reviews from **Benassi, Krippner,** and **Sanders** (but not from **Pinch**). However, such cooperation requires a modicum of mutual trust. The trust between parapsychologists and their critics received a major setback a few years ago when Randi planted two fake psychics at a major parapsychological research laboratory and then proceeded to make exaggerated claims to the media about the success of the hoax (cf. Randi 1983a; 1983b; Thalbourne 1983; Truzzi 1987). This resulted, at least indirectly, in the dissolution of the laboratory.

If **Gardner,** who applauded this deception (Gardner 1983), is seriously interested in promoting cooperation between **Schmidt** and **Hyman,** we find it odd that he should wish to publicly identify himself with such a proposal. In any event, we have obtained written assurance from Schmidt that he has not only saved the data from his precognition experiment (Schmidt 1969b) for almost 20 years, but that he would be willing to share them with Hyman.

2.15. If . . . then. . . . If omegic processes exist, then why can't a group of psychics move an arrow in a bell jar (**Gardner**), why don't split-brain patients use such processes to communicate interhemispherically (**Beyerstein**), or why can't we cure cancer (**Feder**)? Using similar logic, we might ask how it is that if memory exists, a random sample of Durham Ph.D.'s can not reliably recall whether or not it was raining in town three weeks ago? The point, of course, is that the failure of an effect to occur under one set of conditions cannot be used to refute its occurrence under some other set of conditions. Likewise, we think that under close examination **Navon**'s statement that "the existence of an effect that shows up much less often than it fails to show up cannot be claimed without specifically accounting for the alleged failures" proves to be a non sequitur, unless he means that the failures would show the successes to be statistical flukes (i.e., type 1 errors). We simply do not know enough about psi to explain why it occurs under some circumstances and not others. We are trying to find out how to explain it when it does occur.

2.16. Practical significance. The commentary of **Cicchetti** was sent to us earlier as a referee's critique and was the stimulus for Section 6 of our target article, which we feel addresses his main point. Parapsychologists (including, ironically, Schmidt) have themselves developed various measures of "psi efficiency" (Beloff & Bate 1971), although these have seen limited application. We agree that it is useful to have some way of assessing the practical significance of a given effect size, but it is not clear from Cicchetti's commentary what rationale underlies his assignment of labels such as "poor" and "excellent" to various levels of kappa. Don't such judgments depend, among other things, on the particular application envisaged?

Finally, we fail to understand, for ESP at least, why **Gardner** thinks that amplification is more "plausible" when based on a small number of guesses from each of a group of subjects than when based on a large number of guesses from one subject.

3. Theoretical/philosophical issues

In our target article, we dealt minimally with theoretical concerns of psi research because we felt that empirical questions are of greater relevance at this stage of development in the field. Our reference to the noise reduction model, for example, was merely intended to emphasize the fact that the field is not strewn with incoherent and chaotic masses of disparate results, and that the finding seems to generate hypotheses that provide for a viable program of research. We are not oblivious to the importance of theory testing in the field, however.

Traditionally, there has been a degree of ambivalence about the value of theories. The point that parapsychology has "a factual basis on which there is yet to be built a great theory" (Scriven 1976, p. 73) has been made quite often. "The theoretical side of psychical research," wrote philosopher H. H. Price (1949), "has lagged far behind the evidential side. And that, I believe, is one of the main reasons why the evidence itself is still ignored by so many . . . highly educated people" (p. 20). This view was, however, contested by another philosopher, C. W. K. Mundle (1976), among others.

J. B. Rhine, who was at the forefront of parapsychological research and dominated the direction it would take for over half a century, himself felt that theorizing would shackle the research and that theories are closed gateways. By emphasizing that what we need are hypotheses that are experimentally testable, Rhine sought to provide parapsychology with a firm empirical foundation. (See Rao, 1977, for a review of theoretical attempts to explain psi.)

3.1. Does omega necessarily imply dualism? The implicit suggestion of **Beloff, Gergen, Parker,** and **Tobacyk** seems to be that omega does imply dualism. **Dybvig** comes very close to saying this, but he leaves materialism the "out" of expanding its conception of neural processes. **Braude** and **Flew** maintain that omega does not imply dualism, although Flew notes that the BLPs are based on a dualist worldview. **Navon** explicitly rejects the converse of the statement – that dualism implies omega – so we suspect he agrees with Braude and Flew, as apparently does **Beyerstein.**

We agree with **Braude** et al. For example, the "physical theories" of psi discussed by **Krippner, Stanford,** and **Vassy** violate one or more BLPs but clearly are not dualistic. We also note that only the last of the four major BLPs is explicitly dualistic (cf. Broad 1962), and thus the only revision that may be needed to render the BLPs metaphysically neutral for definitional purposes is to drop this one from the list. Because all psi anomalies that might violate this BLP also violate one of the others (e.g., mediumistic communications), this move would have no objectionable side effects that we can foresee.

On the other hand, it is certainly true that dualism has traditionally influenced much parapsychological thinking and that even some modern omegic views, such as Walker's (1984b) theory, are explicitly dualistic. However, most omegic conceptualizations represent what **Braude** calls "level-of-description dualism" and imply no metaphysical commitment at all. We do not think scientific research can in principle validate a metaphysical proposition like dualism in any strong sense. Whether or not the outcome of psi research ever *suggests* dualism will depend on the nature of the particular omegic theory (if any) that is eventually validated. Until then, to discuss the metaphysical implications of omega is premature.

3.2. Implications for society. Although, as **Utts** and **Tart** point out, omega is bound to have important societal implications, the nature of those implications, just like the metaphysical implications discussed above, will depend to a considerable extent on what kind of omegic theory (if any) is ultimately confirmed. For example, none of the six characteristics of the transcendental worldview listed by **Tobacyk** would necessarily follow logically from the confirmation of an omegic theory of psi. To give just one example, precognition could (under certain circumstances) be used to prove that the future is predetermined (i.e., determinism).

On the other hand, the cultural consequences of theories need not follow from them logically, and some writers may well use psi research (and have already used it) to support a transcendental worldview. However, this problem is not unique to parapsychology. Some brain research has been used more heavily for this purpose than has psi research (cf. Ferguson 1980).

Clearly, *all scientists have a responsibility* to do what they can to prevent unwarranted inferences from being drawn from their research. On the other hand, the possibility that psi research might have societal consequences that some find distasteful should not, in our opinion, be used as an excuse for censuring *responsible* open-minded discussion of omegic hypotheses in the scientific literature (or, for that matter, in the popular literature). **Navon** may have taken the opposite position at the end of his commentary, but we are not sure. **Wolins** goes further by attempting to discourage funding of psi research on the grounds that it might mislead the public about the nature of science. Even if this were true, it would not justify suspension of open-minded inquiry (which would be the practical consequence of a funding cutoff). Should split-brain research not be funded, for example, because some people misconstrue it as supporting the independent existence of mind?

3.3. Implications for conventional theory. We suspect that part of the impetus behind the perceived need for antecedent probabilities (sect. 2.2, this commentary) is the view that the validation of omegic effects would somehow require drastic revisions or even the overthrow of conventional scientific theories. We found some indications of this concern in the commentaries of **Beyerstein, Bunge, Feder, Railton, Sanders,** and **Tobacyk.** We agree with **Braude** and **Spanos & de Groot** that such an *outcome is unlikely, or at the very least unnecessary.* First, the levitation of one's pencil (to use a now familiar example) would not necessarily mean that the law of gravitation does not apply (gravity may merely have been overcome by some stronger force, which need not violate conservation of energy; cf., Braude). Even if the laws of gravity were "violated," why should one abandon them (or any other widely useful laws or theories) simply because events have been shown to occur outside their domain? What is threatened is not the validity of these laws or theories, but their universality. At "worst," conventional theories of physics might have to be subsumed under some larger "omegic" theory that would explain omegic as well as conventional effects, much as Newtonian mechanics was subsumed under relativity. There are distinguished physicists such as **de Beauregard** and **Josephson** who do not see a fundamental incompatibility between parapsychology and physics, and even Walker's (1984b) explicitly dualistic theory of psi makes a point of retaining all known quantum mechanical equations. Although unprecedented (in the physical sciences) and hardly ideal, there is no logical reason why predictive, internally consistent, but totally independent and incommensurable conventional and omegic theories applicable to nonoverlapping bodies of data might not coexist, at least for a period of time.

We note that both these types of omegic theory, even if highly predictive, would violate at least some of the demarcation criteria proposed by **Bunge** in his commentary and thus (presumably) would be pseudoscientific. We conclude that this point illustrates the arbitrariness of Bunge's demarcation criteria. Finally, because no one, to our knowledge, has ever postulated "psi waves," let alone compared them to "gravity waves," we fail to see the relevance of Bunge's last discussion to the debate.

3.4. "Classical" approaches. In contrast to **Vassy,** we think that parapsychologists' lack of enthusiasm for postulating a relationship between ESP and distance has less to do with ideology than with the lack of any credible evidence for such a relationship. As Vassy himself admits, the supporting research "does not always [we would omit the always] meet present-day standards of parapsychology." Although psi data are at present far too noisy to support confirmation of anything like an inverse square law, we think that if psi were indeed distance dependent, then some better indications of this fact would have surfaced by this time. Nonetheless, this argument is hardly fatal, and we strongly agree that the matter should be left open.

The openness of modern parapsychologists to physical correlates of psi, denied by **Bunge,** is amply illustrated by the great deal of interest many of them have shown in the possible theory-based relation between psi and the earth's geomagnetic field (Persinger 1979), an interest noted in the commentaries of **Krippner** and **Stanford.** We feel that, if anything, this work has generated *more* enthusiasm than it merits. As became evident at a symposium held at last year's Parapsychological Association Convention, even the "confirmatory" results lack consistency (compare, e.g., Persinger & Krippner 1986 with Adams 1986), and the studies appear to suffer from methodological flaws far more serious than those afflicting, say, the ganzfeld research (cf. Hubbard & May 1986). Nonetheless, there is enough here to justify continued research.

It is unlikely that embracing "classical physical" theories of psi would make parapsychology more palatable to thoughtful mainstream scientists. Because omegic processes must be channeled through the brain if the subject is to make a response, any theory of psi – even a "dualistic" one – must sooner or later confront at least some of the complexities described by **Beyerstein.** The "classical" theories strike us as especially poorly equipped to meet this challenge. Beyerstein's allusion to Palmer's flirtation with "acausal" models (cf. Alcock 1981, p. 128) – although not quite accurate – nonetheless suggests that Beyerstein would find these classical approaches no less outlandish, and probably even more so, than the more "radical" approaches. The only serious attempt we are aware of in parapsychology to confront the omega–brain interface is the quantum mechanical tunneling notion of Walker (1977; 1984b), which we are not competent to evaluate.

3.5. Parapsychology and conventional sciences. Although it is true that most parapsychologists seriously entertain concepts that are not shared in other sciences, **Bunge** exaggerates when he says that there are no links between parapsychology and other scientific fields. In particular, much of our theory is constrained by a desire to fit in with many (albeit, not all) of the principles of accepted physical theories such as quantum mechanics (e.g., Walker 1984b). Our attempts to integrate parapsychology with cognitive psychology and even psychophysiology are documented in our target article. We believe that such an integration would be far more advanced were it not for the insistence of our critics that we "prove the existence of psi" as a precondition for further inquiry. In any event, we all recognize that some bridges will have to be formed with the rest of science in order to explain psi adequately.

Bunge and **Navon** are simply wrong when they imply that most parapsychologists wish to be separate from psychology. The objectors to this marriage have been the psychologists, not the parapsychologists. One reason why psi has rarely shown up in other scientific contexts might be that other scientists have never bothered to look for it. For example, have they no curiosity about why equipment seems to break down with extraordinary frequency in the presence of certain individuals (cf. Morris 1986)? Several researchers have shown psi effects biasing scores on such psychological measures as word-association tests (e.g., Stanford & Stio 1976) and subliminal perception (e.g., Kreitler & Kreitler 1972). Although such results are not strong enough to threaten conventional interpretations of reasonably strong psychological findings, they might suggest possible contributions to error variance in some psychology experiments. **Pinch**'s observations are certainly relevant here.

3.6. Research based on conventional theory. We are pleased that several commentators (**Alcock, Blackmore, Nadon & Kihlstrom, Parker, Stanford, Tobacyk**) have advocated the study of psi experiences from a conventional theoretical perspective. Better yet, we promise to take their research seriously, even if they confirm hypotheses they believe in. We only ask that such research be viewed as a complement to, not a substitute for, research from an omegic perspective. A healthy *empirical* competition between the two viewpoints can only serve to strengthen both.

3.7. Noise reduction model. It is suggested by **Beyerstein** and **Gardner** that in defending the noise reduction model, we failed to consider contradicting evidence. Because of our reluctance to draw conclusions from isolated studies, we intentionally built our case around bodies of data that exhibit some replicability of the operationally defined effect in question (with the possible exception of the dream studies). Thus, we did not cite isolated studies that contradict the model, just as we did not cite isolated studies that support it (e.g., Honorton et al. 1973; Palmer & Lieberman 1975). However, if a reliable relation were to be found between, say, ESP success and stimulant drugs, we agree that the model would need to be modified or abandoned. However, we disagree with Beyerstein that high motivation and attentiveness are necessarily precluded by the model, provided they do not interfere with the quiet mental state the model specifies.

A point that needs emphasis here is that the apparent elusiveness of psi might be a manifestation of the influence of variables which themselves have opposite effects on normal psychological processes. For example, concentrated mental effort may be seen as detrimental to relaxation. If we can believe yogic literature, however, it is this state of intense concentration accompanied by a completely relaxed state of mind to which the yoga practice is supposed to lead the practitioner. Similar ideas are also found in early Buddhist writings of the Theravada school (see Rao 1978b). Reviewing the studies that attempted to relate electrical activity of the brain and ESP, Rao and Feola (1979) concluded that *effortless* or *relaxed* attention may be conducive to psi manifestations. They related their observation to the report of Das and Gastaut (1957), where yogins they tested showed an accelerated alpha rhythm during their "deepest" state of meditation.

Second, the noise reduction model does not imply that psi occurs only under noise reduction conditions, just that noise reduction facilitates psi, other things being equal. As we admitted in our target article, the lack of control conditions in many of the studies supporting the model is a weakness in the case for it. We nonetheless included these studies as having some relevance because the ganzfeld research, for example, boasts an unusually high replication rate among a variety of experimenters – about twice as high as among the REG experiments, although the latter are generally more powerful statistically. Even if one considers only studies with control conditions, we are confident that the number of statistically significant confirmations far outweighs the number of significant reversals.

Of course, the capacity to integrate a large body of data is only one test a theory must meet. We agree with **Stanford** that other models may also account for the data, and we regret if we implied otherwise. Our purpose was merely to show that parapsychologists are concerned about integrating parapsychology with psychology and that our data fall into patterns that make psychological sense.

3.8. Hypothetico-deductive research. We agree with **Stanford** that more hypothetico–deductive research would be desirable in parapsychology, but we also have some sympathy with those who have felt that the reliability of relationships such as extroversion–ESP should be pinned down before too much effort is exerted in trying to account for them. Also, as **Eysenck** points out, there is *some* hypothetico–deductive research in parapsychology, as well as many studies contrasting psi scores in two or more experimental conditions. These studies, a few of which were briefly discussed in our target article, would appear to meet the minimal prerequisites for theory testing alluded to by **Dawes, Flew,** and **Stanford.**

Blackmore suggests that conventional sciences progress because they stress making predictions from theory rather than strong replication and methodological perfection. Although this was not her point, does it not follow that parapsychology's lack of progress might be the result of its adopting just the opposite strategy in order to placate its critics? Ironically, if one were to forget about replication and the more esoteric flaws and look at parapsychology's track record in the relatively few cases where it has done theory testing, we do not think the record would look that bad (e.g., Stanford 1977a). On the other hand, at least in the social sciences (including psychology), does Blackmore's approach not run the risk that what is accepted as "progress" might actually be built on quicksand? Perhaps what we should strive for is a balance.

4. Conclusion

When conventionalists say "there is no credible evidence for psi," what they are expressing is their supreme confidence that all (ostensible) psi anomalies, whether the results of experiments or people's real-life experiences, can be explained as artifacts or delusions. On what is this confidence based? Clearly, it is not based on research. Only recently have conventionalists begun to call for research on psi "experiences," but this research is not intended to confirm their interpretational framework; instead, it assumes it as a premise. Through their often highly creative flaw assignments, conventionalists have done a very good job of showing that their artifactual interpretations of the better psi experiments are *possible*, but a very poor job of showing that they are *probable*. In the final analysis, their case simply rests on faith – unaffected by countless empirical indications to the contrary – in the universality of conventional laws and theories that, despite impressive successes, have yet to account adequately even for many ordinary mental phenomena. **Child** hit the nail on the head when he characterized the psi controversy as reflecting differences in the value placed upon observation and theory.

We think that the verification of omega will and should require broadly based statistical replication, using sound methodology, of specific predictions derived from internally coherent omegic theories of psi. Is it too much to ask that conventionalists do something comparable before agreeing with **Alcock** that all psi data can be accounted for by artifacts or other conventional mechanisms? The stakes in this controversy (even if sometimes exaggerated) are not trivial, and history will judge us harshly if we allow the weak arguments of the conventionalists to convince us that there is nothing here worthy of serious scientific attention from multiple theoretical perspectives.

1. It is true that **Alcock** has not offered any *specific* explanations of psi data, but explanations exist at many levels of specificity and completeness. For example, if we were to propose that cancer is caused by a virus, it can be said that we have offered an explanation of cancer, even though we cannot specify what particular virus is the culprit.

Author's Response

A to-do about dualism or a duel about data?

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It is a humbling experience to be faced with nearly 50 commentaries from some of the world's most respected advocates and critics of parapsychology. Their responses themselves reflect to a degree the controversy that surrounds parapsychology: My target article was at one and the same time adjudged to be "brilliant" (**Bunge**), "excellent" (**Broch**), and (along with that of Rao & Palmer [R & P]) "outstanding" (**Krippner**), and to be "misleading and inaccurate" (**Tart**), hyperbolic (**Railton**), and ad hominem (**Truzzi**). I shall try to deal with the criticism; let others take issue with the praise! In so doing, I have organized my response around a number of themes that recur in the commentaries.

Worldview. Among those who reacted negatively to my paper, the most common criticism, made by about one-quarter of the commentators (including some who otherwise share my skepticism), was with regard to the subject of dualism. I am described as being "obsessed with dualism" to the extent that it is difficult to evaluate my arguments about the evidence for psi (**Sanders**), as focusing too much on the motives of parapsychologists, (**Benassi**), as considering parapsychology to be a thinly disguised search for a metaphysical ideal and not really a science at all (**Akers, Broughton, Mackenzie, Tart**), and as advocating the materialism of the dominant (orthodox) psychological outlook through an essentially ad hominem attack on parapsychologists (**Truzzi**). **Spanos & de Groot** suggest that from the title onward, my article assumes that parapsychologists' supposed dissatisfaction with materialism *ipso facto* makes their endeavors scientifically suspect, a view shared by **Tart.**

Yet very little of my target article actually had anything to do with this subject: Apart from the title and abstract, and a comment about how the *concept* of paranormality implicitly involves mind–body dualism, there are only two or three paragraphs at the end of the article that focus on the subject of dualism, and these relate not so much to the mindset of parapsychologists as to my speculations about the *persistence* of parapsychology and to my view of the nature of the dispute about psi.

There was clearly some misunderstanding (and obviously I must take the blame for that) about just what it was that I was saying when I discussed a search for the soul. **Sanders**, for example, took my arguments to be directed at the claim that otherworldly souls have been demonstrated; because Rao & Palmer do not make such a claim, he said, my arguments do not strike home. However, I did *not* suggest that any such claim had been made, nor do I believe that the large majority of parapsychologists are trying to demonstrate the existence of *disembodied "souls" as such. My contention is that there* is an implicit search for something that lies outside the scientific worldview as we know it today and that this "something" involves an ability of the mind to interact directly with other minds and with matter, and that it therefore implies an aspect of human personality that is not tied to the body. I used the term "search for the soul" as a metaphor for this.

I similarly used the term "mind–body" dualism as a label for the notion that the mind can act independently in some manner, whether leaving the body, as some suggest, or interacting directly with nature, and this provoked criticism from several commentators. For example, **Braude** accuses me of failing to grasp important issues about reductionism and dualism and never acknowledging that cognitive or intentional phenomena generally – normal *and* paranormal – might simply lie outside the domain of the physical sciences. **Navon** points out that the philosophical doctrine of mind–body dualism does not encompass Rao & Palmer's concept of omega, because mind–body dualism could imply an autonomous psyche, a psyche that is affected by or interacts with the body, but not the idea that a person's mind or spirit can interact with the environment while bypassing the body.

I regret having used the terms "soul" and "mind–body dualism," for they obviously failed to communicate what I was trying to say. Moreover, there is obviously much more to the metaphysics of dualism than my simplistic use of the term suggested. Perhaps if I had used a term such as "a radical dualist (interactionist) position" (as **Beloff** did), or if I had simply stuck to a description of the contrast between parapsychology and "normal" science as a competition between worldviews (**Tobacyk**) or meta-paradigms (**Navon**), I would have avoided difficulty. It is unfortunate if the dualism issue distracted some commentators' attention away from my essential arguments, which concerned methodology and the interpretation of data.

However, I reiterate my view that parapsychology reflects a worldview that opposes the predominant materialistic worldview of contemporary science. I am supported in this view by no less an authority than **Beloff** when he states in his commentary that psi contradicts physicalism, the doctrine that everything must ultimately be explainable in terms of physical laws. Consider **Schmidt,** for example. Although he rejects the claim that his work is an attempt to establish the reality of a nonmaterial aspect of human existence, he then admits that he is attempting to show that current physics fails in systems that involve human subjects. This comment would support my (unfortunate) "soul" metaphor, because if mind/thought/personality can interact with matter independently of the body, it would seem that the personality in some way exists separately from the body.

Some commentators read into my remarks that I believe parapsychology to be unscientific on the basis of this presumed difference in worldviews. **Benassi** and **Donderi** interpret me as suggesting that parapsychologists fall prey to tricksters and conduct experiments because they are not searching for scientifically validated truth but for the soul. **Tart** argues that interest in the notion of a soul is not inherently unscientific in any case, and **Woodward** argues that monism and dualism are metaphysical positions allowable to one making any scientific claim. Yet nowhere did I say – nor would I suggest – that parapsychologists are poor scientists simply because they take a dualistic or any other metaphysical position. Indeed, it seems to me that some commentators were so upset by any reference to "soul" or "dualism" that they took for granted that I was being pejorative. If it is inherently "unscientific" to be searching for evidence of either, let them say that themselves.

Donderi argues that I postulate a priori a materialistic universe that precludes the existence of paranormal phenomena, and **Tart** feels that it is my certainty about, and attachment to, current physical theories that stimulates my attack on parapsychology in the first place. Yet nowhere did I state that the dualistic hypothesis has to be wrong or that it is to be eschewed by those who practice science. Nor am I emotionally unable to accept some kind of dualism, should evidence and logic demand it. Either reality is dualistic or it is not. Either psi, in R & P's omegic sense, exists or it does not. Ironically, **Child** sees me as the theory truster and R & P as the observation trusters. I happen to believe that exactly the opposite is the case: I cannot find any data that are persuasive, and therefore I see no reason to change my materialistic worldview or to believe that psi exists. I would argue that if R & P *are* data-driven, then, unless there are some substantial breakthroughs in the near future, we should expect them to follow in the footsteps of other parapsychologists such as **Blackmore** and **Akers,** who have in a very real sense moved outside parapsychology to become critics

I want to stress that I did not by any means intend to imply that parapsychologists want anything less than to find "scientifically validated truth." Of course they do; the argument is whether or not they are following sound scientific procedure in their attempts to do so – whether, for example, because of unfalsifiability of hypotheses (through, say, the psi version of the experimenter effect), they have gone astray. Nor should parapsychologists' data be disregarded because of any particular worldview. In the past I have written (Alcock 1985) that

> it is important to note that it is not because of Newton's testimony or Newton's beliefs that his classical laws are accepted as principles underlying certain aspects of nature on a macro level. It is not even because of his data, some of which was fudged (Westfall 1973). Nor is it because of Einstein's philosophy that most scientists now believe that his theory of relativity is substantially correct. It is not because of Pasteur's beliefs or motivations or even his data that we believe that pasteurization of milk is efficacious and essential. Clearly, personal motivations are irrelevant here; in all these cases, we believe because subsequent empirical testing of these ideas has repeatedly supported them. On the other hand, we would be wise to refuse to accept any of them if only people who were already persuaded of their reality could find empirical support.
>
> In parapsychology, as in these cases just cited, the motivations of the researchers are also irrelevant to the evaluation of their claims. They become important only in our understanding of the *persistence* of the quest. (p. 561; italics in original)

I continue to hold that view, and I am sorry if that did not come through clearly in my target article. On the other hand, **Bauslaugh** is right in stressing the importance of credibility in those cases where *only* believers in a phenomenon are able to produce evidence for it. If the only scientists whose data go against the view that smoking causes cancer are those who work for tobacco companies, then we may have reason for concern. If the only researchers who are able to detect psi are those who are persuaded a priori that psi exists, then perhaps we also have reason for concern. All may be excellent scientists; all may inadvertently bias their data and their interpretations as a result of their expectations. The same, of course, applies to those, including skeptics, who may have other belief orientations. That is why the crucible of scientific debate is so important and why replicability by others who do not share the a priori belief assumes such a prominent role in such cases.

Before leaving this subject, I believe that the relationship of belief systems to parapsychological endeavor is one that is worthy of empirical examination. Yet, as

Krippner correctly points out, I have no hard data to support my speculation about the reasons for the persistence of parapsychology. In this vein, **Akers** refers to Allison's (1973) survey (which was unknown to me because it has not been published) of the membership of the Parapsychological Association in which 43% disagreed with the statement that "the results of parapsychological research clearly indicate that there is a nonmaterial basis of life or thought." He suggests that those data undermine my argument. I would disagree. First, Allison's data refer to the evaluation of the evidence, not to the motivation to pursue parapsychology. Second, I would imagine that someone like **Beloff** – who in his commentary argues that the importance of parapsychology is that it alone can provide the relevant empirical evidence in deciding between an epiphenomenalist and a radical dualist position on the mind–brain relationship – would disagree that the evidence *clearly* supports that view (see his commentary). Therefore, those who disagreed may well, as **Akers** did acknowledge, simply be unconvinced by the evidence. What is more important, I would argue, is that 56% of the Parapsychology Association membership actually *agreed* that the evidence is already in. To me that is astounding. Over half the membership in the leading professional parapsychology body is already persuaded about a nonmaterial basis for life or thought.

The problem of definition. Several commentators took up the issue of the definition of psi. Indeed, one difficulty that I have encountered in preparing this response is that most commentators seem to implicitly implicate the paranormal when they refer to psi, which is quite out of keeping with the approach that R & P formally advocate, albeit in line with what most parapsychologists themselves do in their own writing. Although psi was originally conceived as a neutral term for paranormal phenomena, it now seems that one must differentiate between "psi" and "parapsi."

Sanders argues that R & P have tried to observe the convention, coming, he says, from Hyman and Honorton (1986), that psi is to be understood as a communication anomaly. He contends that I have failed to do the same and that it is thus difficult for him to see how my arguments about dualism can be brought to bear. However, psi as a communication anomaly has yet to become the conventional usage. **Hyman** points to this when he refers to R & P's definition of psi in terms of anomalies as "unorthodox"; R & P went on to qualify this definition by referring to anomalies as "ostensibly paranormal." Indeed, as both **Railton** and **Akers** observe, paranormality crept back into R & P's concept of psi even after they had formally thrown it out. Railton shows how the concept of psi used both by Hyman and Honorton (1986) and by R & P departs from simple anomaly and leads into the paranormal.

R & P introduce a new term, "omega," to which they assign the paranormal aspects of psi. However, as **Navon** remarks, this new term is unlikely to be helpful because it is another blanket label that can be applied to a wide range of phenomena. I agree with **Stanford**'s suggestion that this term not be used, for it is already loaded with other meaning that will only serve to further confuse. There is more than a little irony in the fact that whereas I discussed the persistence of parapsychology in terms of a search for the soul, R & P chose the term "omega," with its religious and spiritual connotations usually associated with death (as in *Omega: The Journal of Death and Dying*), to describe the paranormal.

How can the 56% of the membership of the Parapsychology Association who believe that there is already clear evidence for a nonmaterial basis of life or thought be content with a definition of psi that directly undermines that view by speaking only of anomalies? **Parker** makes the point clearly: By redefining the field as the study of ostensible psychological anomalies, one may blur and compromise the issues. He seems to be saying that this would be akin to throwing the baby out with the bathwater, for he believes that the past 50 years of research has done more than simply point to anomalies of one sort or another. Parker wonders whether parapsychologists would not do better simply to define parapsychology as the study of phenomena that apparently relate to the nature of the interaction between consciousness and the brain by suggesting that consciousness can directly influence external events. This would certainly make it much clearer just what is being discussed.

Anomalies and explanation. Obviously, the crucial question is whether or not there are "communication anomalies" that require explanation. In **Child**'s opinion, R & P have made a clear case for the presence of anomalies. In **Hyman**'s opinion, on the other hand, this case remains to be established, and he sensibly calls for such anomalies to be referred to as "ostensible" psi anomalies, a term that by coincidence **Palmer** introduces in his commentary. I want to underscore the fact that there have, to my knowledge, never been anomalistic claims, from either mainstream psychology or natural science, that seem to imply psi or the paranormal. (Incidentally, I find strange **Pinch**'s recommendation that in order to win recognition from mainstream science, parapsychologists should demonstrate that equipment from other areas of science does not work as expected or needs to be modified, or they should convince orthodox psychologists that they should control for psi effects in their own research. Surely this is begging the question.)

Some parapsychologists argue that psi, whatever that means, is at least as believable as the explanations that critics contrive, if not more so. **Parker**, for instance, believes that "the probability for *all* these contrived explanations being valid is beyond the limit of (my) common sense." **Palmer** expects skeptics to provide specific explanations for psi data, and argues that conventional alternatives are so deficient that even their strongest proponents are reluctant to acknowledge them as interpretations of the data. I do not know what he means by this, for, as **Beloff** remarks, skeptics do not need to demolish R & P's evidence, nor do they need to provide plausible counterexplanations, nor are accusations of fraud necessary. All that is necessary is to point to the methodological shortcomings of the experiments. In the same vein, **Hyman** argues that there is no need to try to provide conventional explanations, because parapsychologists have yet to demonstrate the existence of anomalies. Hyman makes the excellent further point that in the absence of adequate theory and reasonable specifications, there is no reason to expect that an alleged departure from chance baseline in one study is the same "thing" that produced another alleged departure from chance in another experimental situation.

Akers shares my view that psi effects arise from the normal "garden variety" artifacts that appear in unguarded moments in other social science research. **Truzzi,** on the other hand, thinks that something "interesting and probably new" is going on, but suggests that this may be due to new kinds of artifacts yet to be discovered. It is not clear to me how Truzzi would find reason to go beyond Akers's position at this point.

Questions of theory. Two major underlying concerns regarding theory and parapsychology emerge from the commentaries. On the one hand, there are those who argue, as does **Bunge,** that genuine sciences are members of a closely knit system of partially overlapping research fields, and that new knowledge claims should be viewed with some suspicion if they are inconsistent with what is already taken to be known. **Bunge, Navon,** and others argue that the omega hypothesis conflicts not just with the data of science but with its foundations. On the other hand, there are those who, like **Adamenko** and **Utts,** worry that a focus on such consistency will lead us away from important discoveries that could radically change that worldview. However, the notion of psi had been around long before many of the "impossible" things of the past became "possible," and it rests much as it did a century ago on very controversial evidence. Both psi waves and gravity waves may someday become part of scientific "reality," but as Bunge eloquently points out, the latter have the advantage of already being consistent with a considerable body of theory and data. Despite **de Beauregard**'s radical contention that the theoretical formalism of modern physics implies paranormal phenomena, a view that **Josephson** seems to support, I believe that most scientists would agree that the fact that psi does not "fit in" with existing knowledge, or with our beliefs about causality, is good reason for assigning it a low a priori likelihood of existence. While we are on the subject, **Braude** argues that I am completely wrong in suggesting that it is a logical principle that a cause cannot precede its effect. However, another philosopher, **Flew,** argues that backward causation admits to conceptual incoherence and self-contradiction.

Beyerstein provides an excellent review of some of the difficulties that psi (of the paranormal variety) poses for neuropsychology, or, more to the point, that neuropsychology poses for psi. I urge parapsychologists to take his comments very seriously. The body of evidence built up over the years by psychologists and neurologists about the functioning of the mind/brain cannot simply be bypassed or dismissed. Beyerstein points out that the monist position was at one time a rather radical one that came gradually to be accepted because of the weight of the evidence, and not because researchers wanted to avoid dualism. Beyerstein's commentary also, I believe, speaks to **Parker**'s suggestion that parapsychology is clearly in tune with the contemporary view in psychology that consciousness and experiencing have a steering function in the organism. The contemporary view gives no succor to parapsychological hypotheses.

Questions of methodology. Each research discipline develops its own methodology or borrows from that of its neighbors. Often, the methods that evolve in one discipline are not applicable in other disciplines. For example, the controlled laboratory experiment as used by psychologists is totally inappropriate to the study of astronomy; the methodology of chemists is of little relevance to a field botanist. This gives rise to two very important considerations:

1. We cannot judge whether or not a field of research is "scientific" simply on the basis of its methodological approach. There must be an appropriate match between its methodology and its subject matter (**Hyman,** personal communication). **Tart** suggests that there is nothing unscientific about a search for the soul. I believe that although we should not, of course, decide a priori that the quest is not scientific, this claim cannot even be evaluated without looking at the methodology involved. Using radio telescopes to scan the heavens for the souls of our ancestors would presumably not be scientific, unless there was some good theoretical reason to think that souls might be located by such a method. Presumably, then, one should begin with some axioms about the nature of the soul, formulate hypotheses on this basis, and then test the hypotheses using techniques that appear appropriate given what one believes about the phenomenon. What axioms do we have in the case of psi? What methodology would be appropriate? If psi involves the paranormal, then it may be that the methodology of experimental psychology which has been taken over by parapsychology is inappropriate. For example, **Dawes** calls for experimental control of the conditions that are supposed to produce or enhance psi. This is what one would try to do in mainstream psychology, but Dawes recognizes the virtual impossibility of doing this with psi because there is no body of theory that specifies when psi should not occur. **Braude** recognizes the problem as well when he argues that the conventional experimental methods in parapsychology are powerless to reveal anything interesting about the phenomena, except perhaps that they exist, because if we take psi (i.e., the paranormal) seriously enough to test for it, then we must give up the ideal of the blind or double blind protocol. I would go further than Braude and argue that to demonstrate even the very existence of paranormal phenomena is problematic for the same reason.

2. Because methodology tends to be discipline specific, and because the sources of possible error are somewhat different from discipline to discipline, it is not always easy for someone trained outside the discipline to evaluate the methodology used within the discipline. Geologists are not concerned with placebo effects; therefore, if a geologist were to evaluate clinical studies of a particular drug, he might take the study to be carefully designed and executed even in the absence of controls for placebo effects. Physicists and philosophers have little experience in working with human subjects and can quite often be very naive about the many ways in which subtle influences can confound data (just as some psychologists can appear naive about philosophical concepts such as mind–body dualism!). Moreover, because effect sizes are typically so small, and because even minimal communication between subjects in ESP experiments must be eliminated, there are special problems of experimental purity in parapsychology that many psychologists know little about. My point here is that scientists and philosophers

should not automatically assume that they can competently evaluate the findings of parapsychological research unless they have done the necessary homework and carefully explored the methodological traps and statistical pitfalls specific to parapsychology. It is not enough to let the data speak for themselves; they always need interpretation. Thus, when **Broughton** argues that many scientists – including quite a few whose credentials in more orthodox fields are unassailable – read the same parapsychological reports I read and come to the conclusion that there is something to be investigated, I find that general comment no more informative than if he were to say that outstanding geologists and astronomers have passed judgment on medical controversies. That is not to denigrate these scientists; it is simply to say that expertise in one area does not automatically lead to critical acumen in another.

With regard to alleged flaws, **Broughton** chides me by saying that all experiments everywhere are flawed. That may be true. However, as **Blackmore** points out, even if parapsychology's methods are often as good as psychology's (which, incidentally, may be because of weaknesses in psychological research), the difference is that psychology can tolerate considerable error and still progress. Remember, too, that the essential claim of the parapsychologist is that *all conventional sources of influence that might have brought about a higher than expected correlation have been ruled out.* If they have not, then by definition there is no anomaly. Conventional psychological research certainly does not involve the demonstration of phenomena which otherwise seem to conflict with the scientific corpus of knowledge and which in order to be demonstrated require such stringent control of extraneous variables, not all of them necessarily known at any given time.

More and more, parapsychologists are entering into debate about whether or not a flaw is serious enough to vitiate the results of an experiment. Sometimes it is argued (as by **Palmer**) that the onus should be on those who point to the flaw to show empirically that it could in itself produce the effect, and that flaws found by critics need to be evaluated against the same standards of plausibility and empirical evidence as any other scientific explanation. In other words, the critic should be able to demonstrate that the flaw could have produced the outcome. I can think of no area other than parapsychology where anyone has attempted to place the onus on the critic to demonstrate that an acknowledged flaw was both a necessary and sufficient cause of the effect. Psychological researchers do not argue over whether or not a particular flaw was basis enough for the results. I presume that physicists and chemists and medical researchers do not argue, particularly when controversial claims are involved, that the flaws were not bad enough to produce the observed effects. Presumably, they repeat the study *without the flaws.* Why should anything less be expected of parapsychologists?

The Schmidt studies. I am accused by **Schmidt** of never having read his paper firsthand, a point that is amplified by **Broughton,** who accuses me of depending on **Hyman** and **Hansel.** In reality, I have carefully read all the published papers of Schmidt of which I am aware. However, I do apologize to Schmidt for my remarks about lack of supervision. Although there was inadequate supervision of subjects in some of his studies (Schmidt 1970c; Schmidt 1978; Schmidt & Pantas 1972), this has not been a general problem, and I was in error in suggesting that it was.

With regard to the source of randomness, **Schmidt** indicates that he has used the same type of generator for nearly all his experiments, although he admits that there have been changes in the circuitry as time has gone on. In fact, Schmidt (1969b) used a modulus–4 random event generator (REG) based on radioactive decay. Schmidt (1969a) used the Rand tables. Schmidt (1970a; 1970c; 1978; 1979a) used a binary output REG based on radioactive decay. Schmidt and Pantas (1972) went back to the modulus–4 REG/radioactive decay. Schmidt (1973; 1976) used an electronic noise REG. Schmidt (1974) compared two different REGs. Schmidt (1979b) used an electronic die based on radioactive decay. Schmidt (1981a) used seed numbers for an algorithm which were generated by a binary REG based on radioactive decay. Schmidt (1985) and Schmidt et al. (1986) used a computer that was driven by radioactive decay. So, although Schmidt has been pretty consistent in using radioactive decay (or sometimes electronic noise) as the ultimate source of randomness, the apparatus used to "capture" this randomness has varied considerably, leading me to reiterate that the REG, which is *more* than just radioactive decay, has varied from experiment to experiment. Not only is a given generator not explored in a consistent way, but the series of experiments themselves do not build upon one another. Furthermore, in many of these studies, there are clear methodological shortcomings apart from concerns about randomness. For example, Schmidt has often served as his own subject, and in one case was really the only subject. In many of the studies, there are varying numbers of trials or sessions per subject, and these are usually combined. Schmidt has generally worked in isolation from others, and his raw data, with little exception, have not been available for scrutiny. **Gardner** urges that **Hyman** be allowed to examine the raw data from the Schmidt 1969 experiments. I hope that Schmidt (and Hyman) will agree to this suggestion.

Akers questions my concerns about the generator bias problem in the **Schmidt** experiments, stating that Schmidt conducted extensive control runs and that even when these were cut into small segments, no bias was evident. My concern is that bias is not necessarily constant. Subjects were typically allowed to play with the machine and to switch into and out of "experimental mode" when they wished, allowing for the possibility of exploiting short-term biases, even if, over the long run, no bias is evident. As for the cutting into short segments, I presume that Akers is referring to Schmidt (1970a), where Schmidt cut the control runs into blocks of a size similar to test runs. He then used a goodness-of-fit approach to show that there was no bias. Unfortunately, this just tells us that there was no bias in the frequency distribution, based on short blocks. It does nothing to show that there were not, for example, short runs shortly after startup in which 4s, say, predominated and other short runs later on in which, say, 3s or 2s or 1s predominated. Overall, the frequency distribution by blocks can be unbiased, but so long as the subjects can opt in and opt out of the experimental series, they can still learn to exploit the underlying bias, if it exists. Furthermore, because there may have been environmental differences between the times at which experimental and control runs were made (e.g., different loadings on power mains if test runs were run by day and control runs by night, as happened in some studies; differences even due to the presence or absence of human beings if the shielding was poor, and so on). With regard to the excess production of 4s noted in a precognition experiment (Schmidt 1969b), when subjects were supposed to be predicting, not influencing, the outcomes, it would only have been proper to balance the attempted production of 4s with the attempted production of 3s, 2s, and 1s, rather than using what was apparently the very same random event generator in a later psychokinesis experiment (Schmidt & Pantas 1972), where psychokinetic success required that the subject influence the machine to produce 4s (not 3s or 2s or 1s).

In his commentary, **Schmidt** describes all sorts of correlations and so on regarding the control runs of his generator. This does not tell us that *at the time the subject was responding there was no bias.* Would it not be considerably easier just to listen to critics like **Hyman** and **Hansel** (see my target article), and now **Gilmore** and **Dawes,** and do the studies in a way that eliminates this bias?

Fraud. I ignored the problem of fraud in my target article. However, it should not be totally overlooked, for although it is a problem in all research areas, it is potentially more serious in parapsychology given the lack of strong replicability. This has long been of major concern to **Hansel. Broch** argues that fraud is a *major* source of significant results in parapsychology, and even **Akers,** whom I know to be very conservative and respectful in his criticism, points to fraud as well and suggests that there is a strong possibility that psi arises primarily or entirely from a combination of experimenter error and fraud. Because the number of researchers claiming clearcut evidence is not large, he says, fraud would not have to be very widespread either.

Statistical inference. Several commentators focused on the statistical evaluation of parapsychological data and the applicability of classical statistical inference. This is an important consideration, because the modern case for psi rests squarely on a statistical footing. **Dawes's** example shows vividly how the same data can lead to either significant or nonsignificant results depending on what one takes to be the unit of analysis. **Gilmore** has provided an excellent discussion of the problems associated with using "randomness" as a baseline against which a subject's guesses are compared. Gilmore also wisely questions the appropriateness of using classical statistics to evaluate data when the effect size is extremely small, and he urges parapsychologists to use the randomization test to eliminate this important concern. He also presents an appropriate experimental design for REG studies, which parapsychologists should consider very seriously. Dawes proposes a procedure for REG studies that is related to that of Gilmore in that, again, the calculation of exact probabilities is involved without making any guesses about the form of the distribution.

Obviously, parapsychologists attempt in each experiment to demonstrate the presence of psi, regardless of how psi is defined. Statistically significant deviations from chance are the usual indication that psi has occurred. If it were not that the presence or absence of psi by itself was very important to these researchers, there would be no need at all to discuss any of the literature that R & P cite because the effect sizes are so small. For example, if similar effect sizes were found in cancer research or in the search for a better glue, no one would pay any attention to them. It is in this vein that **Glymour** draws attention to the fact that the p value is not a measure of the size of the effect. In general, the effects are miniscule in ESP experiments even though the p value is often extremely impressive. All this means, of course, is that one can be extremely sure that the null hypothesis is not true; but this gives no indication of *why* it is not true. (If all cars made by company X are exactly one micron longer than cars made by company Y, then the p value associated with rejecting the null hypothesis that the cars are of equal length would have an infinite string of zeros after the decimal point because the null hypothesis of no difference is absolutely false. In practical terms, however, such a difference would be unimportant.) **Cicchetti** presents a method for evaluating the magnitude of a psi effect and applies it to **Schmidt's** lamp-lighting experiment to show that the highly significant statistical effect is of utterly trivial consequence from a substantive point of view. **Utts** recommends the use of confidence intervals rather than exclusive reliance on p values, because a confidence interval communicates to independent readers just how large or how small an effect is; this would be excellent advice for psychologists in their research as well.

Yet, of course, no matter how small, if there really is some paranormal effect, it is not insignificant from a philosophical and scientific point of view. On the other hand, as **Navon** says, because experimental controls can never guarantee that an experiment is not systematically biased, one should expect that any null hypothesis can be rejected with a large enough N, even if there is only a very small bias present. He contends that the smaller the effect, the more likely that it is due to an artifact.

Utts also argues that when effect sizes are small, very few studies should be expected to yield significant differences from chance, and that a typical ganzfeld study should be expected to obtain significant results only about one third of the time, even if the true hit rate is 38% as opposed to a chance rate of 25%; she suggests that replications should not necessarily be expected to confirm the original results. This statement worries me because it could be taken to suggest that we should not be too concerned just because the replicability rate is low; the replicability rate is also certainly going to be low if there is no real effect.

Misleading the reader. A small number of commentaries accuse me of misleading the reader (presumably deliberately) about parapsychological research. **Tart** mentions my "many misleading statements," but instead of addressing them, he deals only with two aspects of my apparent philosophical position. **Hövelmann** takes me to task for not pointing out that many of my criticisms of parapsychological reserach are to be found within the parapsychological literature itself. To the extent that this is true, and I do not disagree, I take it to be support for my position.

There are four specific criticisms about selective or biased reporting that require detailed rebuttal:

1. The Hyman–Honorton exchange. In my discussion of the Hyman–Honorton exchange, I quoted Hyman and Honorton (1986) regarding their conclusions about the data base. In so doing, I left out three sentences, not in order to mislead, but in order *not* to mislead. These three sentences were as follows:

> Although we probably still differ on the magnitude of the biases contributed by multiple testing, retrospective experiments, and the file drawer problem, we agree that the overall significance observed in these studies cannot reasonably be explained by *these* selective factors. *Something beyond selective reporting or inflated significance levels seems to be producing the nonchance outcomes.* Moreover, we agree that the significant outcomes have been produced by a number of different investigators. (p. 352, my italics)

Hövelmann and **Nelson & Radin** contend that those three sentences strongly contradict the general impression I convey about the status of parapsychological evidence. On the other hand, **Spanos & de Groot** fault R & P for *only* reporting the part that I left out, making it look as though Hyman came to agree that there were genuine anomalies in the ganzfeld data. **Railton** and **Krippner** both assert that both R & P and I have reported just those parts of the Hyman–Honorton conclusions that suit our purposes, omitting important aspects that do not.

I cannot speak for R & P, but my own selectivity was based on the following: Because nowhere in my target article did I address selective reporting, inflated significance levels, or the problems of retrospective experiments, I omitted the three sentences in question as completely irrelevant to my discussion. They refer to three specific sources of possible error, and to the fact that Hyman and Honorton agree that these three are not sufficient to account for the outcomes. However, Hyman's position is that various other flaws, and especially flaws in methodology, *could* account for those outcomes (see also **Hyman**'s commentary):

> If psi is responsible for the outcomes obtained in this data base, then the ganzfeld experiment should continue to produce successful outcomes *when the various problems that Hyman pointed out are eliminated.* (Hyman & Honorton 1986, p. 353, my italics)

2. REG studies. Both R & P and I had a section of our target articles dedicated to Schmidt's REG studies. Each discussed the early Schmidt work (Schmidt 1969b) in some detail, and we both made mention of the most recent Schmidt study (Schmidt et al. 1986). Neither paper, in those sections, discussed other Schmidt research, except for a passing reference. However, **Nelson & Radin** single me out for focusing on a handful of Schmidt's early experiments and for failing to mention the many other random event generator studies. They accuse me of ignoring their own body of research. I have carefully analyzed a considerable amount of the Princeton REG research (Alcock 1987). Because there was no space in the original target article for a detailed analysis, nor is there space in this reply, I can only say that I found as much reason to be very concerned about the methodological soundness of experiments giving rise to that body of data as with the other evidence for the paranormal I have examined. For now, let me simply refer to **Akers**'s advice that it would be wise to defer judgment on these REG experiments until more information is published about the experimental procedures.

With regard to **Nelson & Radin**'s comments about my treatment of the May et al. (1980) report, again let me draw from the commentaries: **Hyman** contends that the same criticisms that May et al. levied against REG experiments carried out before 1979 continue to be applicable to the REG experiments that have appeared since.

3. Child's criticism. In his commentary, **Child** indicates that I twice mention his article (Child 1985) without revealing that one of its main points was that critics, including myself, have grossly distorted the most basic facts about the Maimonides research into ESP in dreams. The reader should examine Child's (1985) criticism of me, and the very little that I actually had to say on that subject (Alcock 1981) in order to evaluate the substance of Child's charge. As R & P point out, however, there have been no independent replications of this line of research that have provided significant results, and two major failures to replicate have been reported.

4. The mediumistic evidence. I am chided by **Braude** for avoiding a discussion of the evidence produced by "the best cases" of physical mediumship. Braude argues that these easily resist the traditional charges of error and fraud. This is a matter of opinion, of course, because he is referring to events that took place many years ago with mediums such as D. D. Home and Eusapia Palladino. He accuses me of invoking "one of the least impressive and most generally irrelevant cases of all – that of Uri Geller." That is very easy to say now, but I remind the reader that Geller was taken very seriously by many people inside and outside of parapsychology, and was subjected to considerably more critical scrutiny than people like Home and Palladino before his case became unimpressive and irrelevant. All in all, I believe Braude's argument should be directed not at me but at R & P, the advocates of parapsychology; they failed to make a strong case for the mediumistic evidence, if such a case can be made.

The standards of criticism. Criticism of parapsychology, argues **Pinch**, should *not* be equally applicable to mainstream science, and the critic should be able to specify the conditions under which the criticized work could avoid the criticism. I disagree with the first of these constraints, for my major criticism of parapsychology is that it offers neither theory nor replicable (in the "strong sense") effects. If there is some area of mainstream science where this is equally applicable, then I would level the same criticism in that direction. Pinch argues that I do not suggest how experiments could be improved. I did suggest a control procedure for Schmidt's REG studies, and if space permitted, I could make many other suggestions. However, in general, whenever flaws are found, the best strategy to improve the experiment is simply to get rid of those flaws!

Collaboration between proponent and critic. To raise the level of debate between critic and proponent, **Benassi**

urges both to "play in the same ball game." As an example of this, he refers to the Hyman–Honorton (1986) exchange in the *Journal of Parapsychology*. Whereas I too am pleased by that exchange, it remains to be seen whether it will have much impact in the long run on the way parapsychology is carried out. **Pinch** argues that bringing critics and proponents together to examine evidence is unlikely to resolve anything, and may actually exacerbate the debate, because they are unlikely to share crucial assumptions as to what constitutes a competently performed experiment.

Although I am generally sympathetic to **Benassi's** views, I am puzzled by his suggestion that if after a reasonable amount of time parapsychologists do not provide convincing data for communication anomalies then the scientific community should begin to ignore them. How long is reasonable? Over a century of formal empirical enquiry has been carried out so far.

Finally, I welcome **Parker's** invitation to collaborate in the design of parapsychological experiments. However, mutual supervision of the execution of such experiments is equally important, and I am not sure how practical that would be, given that we live on different sides of the ocean.

In conclusion. I continue to believe, along with **Hyman** (and apparently also **Palmer,** judging by his commentary), that the existence of psi anomalies, let alone the paranormal, has not yet been demonstrated. To those who are new to this debate and do not know whom to believe, I suggest that rather than listening to an acknowledged skeptic like myself, or to acknowledged proponents such as R & P, they reread the commentaries of parapsychologists like **Akers** and **Blackmore** who have developed their skepticism from *within* parapsychology rather than from the outside looking in.

In my view, parapsychology cannot have survived for over a century on the diet of substantiated research findings and theoretical advances that sustains research in other fields, because it has not been blessed with any of these. In order to avoid being misunderstood a second time, I shall eschew my soul and dualism metaphors and argue simply that parapsychology is not anomaly driven, but represents a quest to demonstrate that the materialistic worldview that predominates in modern science is incomplete, and that personality/mind/thought can interact directly with matter. This, *by itself,* does not make parapsychology unscientific. Of course, if the mind–matter interaction can be demonstrated, then, as Joseph Banks Rhine (1943) pointed out, there is at least some reason to believe that the human personality may not be tied to the fate of the body.

Because I, too, am interested in experiences that seem anomalous, I totally agree with **Nadon & Kihlstrom,** who stress that future research needs to elucidate the cognitive nature of anomalous experiences and to examine the situational and dispositional factors involved in their occurrence. As Blackmore (1983a) has counseled, let us get on with the study of anomalous experience and leave the psi hypothesis aside for now, for it only gets in the way. This entire exchange of views, as profitable as it has been for me personally, has had little or nothing to do with anomalous experience, and that is because the focus is not on what people think or feel or experience, but on whether or not psi, presumed by parapsychologists to be very relevant to such experience, can be demonstrated to exist. I believe that that indictment applies to parapsychology in general. I agree with **Truzzi** that inquiry should not be blocked. However, the psi hypothesis is likely, ironically, to hinder progress in understanding the very experiences that parapsychologists say they want to explain.

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